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ORIGINAL ARTICLES.

RELATION OF MATTER AND MIND IN HYPNOTISM.

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BY JAMES F. HIBBERD, M.D., LL.D.

RICHMOND, IND.

All will concede that the present popular consideration given to hypnosis renders it incumbent on the physician who would creditably fulfill his mission that he be posted with knowledge of its phenomena and nature.

In presenting my views on hypnosis, there is no pretense that I am a scientist detailing my original experimental investigations, but rather a philosopher scanning the field of psychic culture, analyzing the products of the labors of experimentalists in mental science, and deducing conclusions by associating and comparing such products with the results of progress in the broader field of physics.

My contention is that the somatic mind is the product of the functioning of the encephalon, the motor of which is vital energy, an isomeric phase of and directly descended from, the oscillatory motion of atomic matter through molecules, protoplasm and tissue metabolism, and I hold that hypnosis, spiritism, Christian science, hysteria, etc., are cognate forms of correlative neuroses, the subjects of which are not up to the fullest development in mental force and balance, the shortage being due to a congenital imperfection in the encephalon or a defective working of the intellectual apparatus.

To fully present the points illustrative of my contention and rehearse the facts and arguments that to my mind demonstrate their verity would require a volume, while my privilege of recital here is limited to twenty minutes reading. I must therefore content myself with a verbal triangulation of the field outlined, and submit for your consideration such of its more prominent features as in my estimation are most likely to interest you.

In a study of hypnosis our first obligation to ourselves is to acknowledge the reality of its existence, and our duty to our *clientele* demands that we be able to point out to them its nature and significance. A weighty fact that should be kept constantly before us is that only a small fraction of the civilized people of the world are susceptible of hypnotization, and not one of this small fraction can be hypnotized without his consent.

The most facile steps in producing a state of hypnosis are for one or more susceptible persons to take an easy sitting position on the invitation of some one in whom each has confidence, known as a hypnotist, who instructs them to fix their gaze and attention on some distinct thing, generally a bit of bright metal, held in front of, and a little above, the eyes and close enough to require a slightly convergent vision to see it clearly, and maintain this position until a sense of drowsiness creeps over them to which they yield and fall asleep, or into a state simulating sleep wherein they are oblivious to everything except the suggestions of the hypnotist. This hypnotist is no particular person but may be any one who by agreeable appearance and bearing wins the confidence of the subjects, nor is there a special virtue in this bit of bright metal; anything, bright or otherwise, which can hold the attention of the subject without exciting his curiosity or other inquisitive sense will answer the purpose. The subject who thus becomes hypnotized I will call a hypnotic.

Suggestion by the hypnotist may be successfully made by touch, by motion, or by words, but not by will power of the hypnotist, except through some act recognizable by one or more of the hypnotic's normal senses. By these means the hypnotic can be made to believe that the tincture of wormwood is a delicious syrup, that the odor of assafetida is the attar of roses; that the notes of a mandolin are the howl of a wild beast; that potatoes are lumps of gold; that contact with ice will scorch his fingers, and so on through the most irrational vagaries. But this line of suggestion has its limits; it must be kept within the bounds of the natural endowments of the hypnotic and the influence of his education, as also the impression of his immediate environment.

These limiting impressions are known as auto-suggestions and are of the highest importance in understanding the possibilities of hypnosis. Mr. Hudson, an attorney, an investigator and writer of hypnosis, in an article (*New York Medical Journal* Jan. 26, 1895,) on the "Relations of Hypnosis to Medical Jurisprudence," and Dr. Cocke in his recent book on "Hypnotism: How it is done; its Uses and Dangers," Boston, 1894," have treated of auto-suggestion quite fully, from which I epitomize as follows: a hypnotic who is a moral man strongly averse to injuring a fellow mortal has a pasteboard dagger placed in his hand when under the deepest influence of the hypnotizer, who directs him to stab the person nearest him in the breast, which he does instantly and without hesitation; then the hypnotist places a real dagger in the hand of the hypnotic with the same instructions, but the hypnotic hesitates and if the hypnotist insists, the hypnotic comes out of the hypnotic state in great agitation. This is due to the hypnotist's instructions to the hypnotic to commit a crime which in the hypnotic's normal state nothing could induce him to do. Notwithstanding his hypnotic state his conscience readily distinguishes between a pasteboard dagger which could do no harm and a steel dagger which would destroy life.

Again, a hypnotist has a virtuous woman for a subject and when he has her under full control makes improper advances to her which she will promptly

resist, and if he presses his purpose will come instantly to the normal exercise of her mind with a shock, probably followed quickly by a hysterical explosion. Any one who submits to be hypnotized can determine before the séance begins, that if certain things are commanded of him while under the influence of the operator he will not obey, and if the certain things are attempted he refuses and can not be forced.

The environment also has a guiding control in the extent to which the hypnotic can be carried. If the séance be a public exhibition and several subjects are presented, there will be a wide difference in the limits within which they can severally be manipulated. In public presentations most of those who offer themselves as subjects do so with the conviction that they are to contribute to the entertainment, and those of them who place no restraint on their actions before the public, obey the hypnotist in every suggestion, however ludicrous it may be; others will refuse to follow suggestions that infringe on their dignity, while still others will decline further advance if the lead crosses the line of their sense of moral rectitude.

The lesson of this phase of hypnosis is that if the hypnotics in any séance should, at the suggestion of the hypnotist commit murder, prove licentious or make buffoons of themselves, neither hypnotism nor the hypnotist would be the essential cause of the misdeed, but at most would only be amenable to the charge of affording the offender an opportunity, and prompting him, to advertise his real character. In his heart he was a murderer, a lecher or a buffoon before he was a hypnotic.

If this sketch of hypnotism be true, it neutralizes the apprehension many excellent people have entertained, that the public was liable at any moment to be confronted with a hideous felony, brought about by a depraved hypnotist suggesting the crime to an otherwise innocent and conscientious hypnotic.

A fortnight since, I read in a Richmond newspaper a statement to the effect that in an Indiana city a man had recently been convicted of murder and sentenced to the penitentiary for thirteen years, mainly on the testimony of a woman who subsequently confessed that her testimony was false and that she had committed the murder herself. This woman was now in a hospital near to death from consumption, and on May 19 the judge went to the hospital with a hypnotist and authorized him to hypnotize the woman that she might tell which was true, her testimony or her confession. Under hypnosis she declared her testimony true and her confession false, the latter being made under duress.

If the mental condition of this moribund hypnotic was such as set forth in this essay, as the ordinary condition of hypnotics, she would declare touching the truth or falsity of her previous statements just as the hypnotist now suggested or as she had determined for herself before entering the hypnotic state. If seriously made, such use of hypnosis would be misprision of criminal jurisprudence, and a travesty of forensic medicine.

Charcot, "Dictionary of Psychological Medicine," states that a typical case of hypnosis traverses three well-marked stages of development, to-wit: catalepsy, lethargy and somnambulism. Catalepsy is the first step and is secured by withdrawing the bright object at which the subject has been gazing, the moment he loses consciousness. On such withdrawal the subject with fixed unwinking eyes continues to gaze at the spot from which the object was removed and, if not disturbed, will remain in that exact position until the contractile energy of his involved muscles is exhausted, when his body will adjust itself obedient to the law of gravity.

In this state, the subject is as plastic clay in the hands of the potter; his arm can be raised to any position and will remain there; his head may be placed awry and will continue thus; his whole body may be molded to any form, esthetic or fantastic, consistent with his anatomy.

The lethargic state may be induced by the hypnotist simply closing the staring eyes of the subject, or he may be allowed to continue his gaze at the bright object and his eyelids will soon droop, then close, his lethargy will be complete and profound and he may so remain, if undisturbed perhaps twenty-four hours, with no other apparent objective evidence of vital activity than is manifested in his respiratory and circulatory apparatus.

The lethargic condition is converted into the somnambulistic state by certain manipulations, especially by the operator rubbing the vertex of the subject with the tips of his fingers or the palm of his hand. The effect of this vertex friction is a well-established clinical fact for which no rational explanation has been afforded.

While the foregoing stages are clearly enough marked in typical seizures, in fresh subjects, the manifestations are mixed and irregular in many fresh cases, and in those who have been frequently hypnotized, the distinction is not observable.

Charcot maintains there is a clear relationship between the hysterical diathesis and hypnosis, and the close observer, after sufficient experience, will not hesitate to accept his conclusions. Conceding the relationship between hypnotism and hysteria, as taught by Charcot, it is as far as we can go, for the phenomena characterizing the two have little in harmony and their sequels are widely divergent—hysteria simulating by times almost every idiopathic pathologic condition, frequently ending in permanent invalidism or structural changes of a hopeless character, while hypnosis but repeats its original distorted features with less and less prompting and its only common mischievous sequel is a perverted intellect.

To obtain a rational concept of hypnosis we must keep distinctly before us that the mind is the product of a multiple organ of most complex organization and complicated physiology. So emphatically is this true that to analyze the structure of this organ and comprehend clearly the fruit of its functioning in the adult human by a study of it in such human, is beyond the powers of the acutest observation and the profoundest comparison.

A year ago it was my privilege to read a paper before this society, devoted to a consideration of the origin and development of man's mental powers, wherein I advanced the proposition that since the time of Aristotle, students of mind had substantially confined their investigations to its structure and manifestations in the adult human, in general terms regarding it as a dual thing composed of a natural and a supernatural moiety, with the result of great diversity of conclusions without satisfaction in any of them. Without questioning the dual character of our minds, I further stated that all its opera-

tions, open to our observation, were due to the vital energy as exhibited by or through our nervous organizations, and that the study of mentality involved only the investigation of the nervous apparatus, the fruit of which is known as the somatic mind, and to comprehend this we must abandon the scholastic method of research and, instead, adopt the modern method of scientific interrogation of nature which discards primary inquiry into the complicated and involved nervous system of adult man, descends through the less and less intricate organizations of the long catalogue of inferior animals, and finally reaches the amœba, held to be the simplest living thing presenting the essential characteristics of the animal kingdom. The amœba is a single cell of undifferentiated protoplasm. It has no muscular system, yet moves; it has no organs of special sense, yet selects its food and seizes it; it has no digestive system, yet consumes food and is nourished; it has no organs of generation, yet propagates its kind and continues its species; it has no nerves, yet all vital activities are accomplished with precision and in the utmost harmony. Here, then, the scientist meets his opportunity to begin the study of the nervous system of man, for this simple one-celled animal is the first link in the extended chain of animal existence, whose final link is the wonderfully complicated human organization. And the succession of living things, from amœba to man, is but an unbroken continuance of added cells and differentiated function, man being apparently the acme of the Creator's intent, but not a uniform product of creative power, for excepting the distinctive points that fix man's place in the classified animal kingdom, the differences of both body and mind among the peoples of the earth, due chiefly to environments or other mundane influences, are so patent that it may be fairly claimed that the contrast between the highest and lowest humans is not less than the difference between the lowest human and the highest simian.

Without further attention to man's organization as a whole, let us give brief consideration to his nervous system—not to its anatomy but its physiology. For the present lesson we recognize three grades of nervous energy, functionally distinct in their typical endowments, yet so intimately blended and interdependent as to require constant recurrence to the type of each to avoid confusion in their conjoined service. The first grade has its genesis in the centers that preside over the automatic activities of the body which include digestion, assimilation and the whole diversified metabolism of the organization, and are but little influenced by the other grades and that little by indirection. The second grade has its seat in that part of the encephalon centrally situated that, in the last few years, has been experimentally demonstrated to be constituted of motor centers. To a considerable extent these centers have been shown to have original motor jurisdiction when excited by either external or internal stimulants. The third grade emanates from the anterior and posterior regions of the cerebrum and includes the perceptive, the intellectual, the emotional and the will powers of the mind, and in its entirety dominates all the voluntary activities of the body, which it does through its command of the other grades of the nervous system. This is the grade that gives rise to the phenomena of hypnosis, and the seeming mystery of these phenomena will disappear if we realize that the mind is not a unit, but is made up of sundry greater departments, sub-departments and minor divisions which may all act in unison, but also under some circumstances one or more of the divisions may function within certain limits quite independently of others and it is here that hypnosis has its genesis.

The actual condition of the mind in hypnosis is the suspension of the will power, except as to the suggestions of the hypnotist which are obeyed implicitly, until obedience involves a violation of the moral sense of the hypnotic as it exists in his normal mental state. This dominant power of the moral sense is a fact of the highest significance, being the witness to a phase of insentient mental operations included by psychists under the terms, unconscious cerebration, mental automatism and reflex action of the brain, that have exceedingly important relations with the entire range of human thought and action.

There is no rationale for the phenomena of hypnosis any more than there is for chemic affinity, gravity or other motorial factor in nature. The fact we need to clearly comprehend and persistently retain is that the full catalogue of vital activities recited, are such, obedient to cosmic law, under which the Creator fashioned the earth and governs all things therein.

Recurring to the amœba, I may recall the fact that it was elaborated protoplasm in which the first vital cell was organized, and that in man there is no elemental matter not found in the world around him, and that of seventy or more elements of the earth only about fifteen enter into the substance composing the human frame. It is the manner of the combination of these fifteen that distinguishes man, and indeed all living things, from other products of the energy of the omniscient and omnipotent Creator. So far as we can now discern, this progress is due solely to the continuous operation of a universal law impressed on elemental matter in the beginning, and as I see it, the status of civilized man and the degree of his enlightenment are measured by the fullness of his recognition of this fundamental law.

It has seemed to me that we shall find the key to all we shall ever know of vital activity, including the expanded intricacies of nervous functioning, by devoting successful study to the dynamics of physics. The striking difference between the variegated movements of advanced animal life and the limited rigid movement of inorganic matter is found in this; in the movements of animal organisms there is witnessed the operation of volition, *i. e.*, the organism, in certain directions, may do things or leave them undone as it elects; even the amœba, the prototype of all the animal kingdom in this behalf, floating in its supporting medium, may simply float as a globular speck of protoplasm or it may project a pseudopod, seize a bit of food and convert it into its own tissue; whereas if atoms of oxygen and hydrogen come into contact under appropriate environment they must unite in definite proportions and form water, and from this destiny there is no escape and no modification, and no atomic matter has other line of action.

Physicists teach us there is but one energy in nature. In our present lesson, reviewing matter in motion, we first meet with this force in the chemic affinity of atoms, and follow it through molecules in increasing number and complexity until they form protoplasm, and in protoplasm still further, until a

cell is organized which presents the characteristics of animal life.

If this review of the progress of matter from the conjugation of atoms to the organization of a living cell has been thorough and true, we will have realized but one energy. The force of chemic affinity in atoms is the same as vital activity in the amœba—nothing added, nothing lost.

It has already been shown that the primal animal cell presented potentially every tangible attribute of man, and that in traversing the wide range of mental force from amœba to man, even by the most enlightened, no new element nor new energy was discovered—simply more cells and higher differentiation of function.

It is the consideration of these patent facts and the logic of affiliated events that enables us rationally to investigate the somatic human mind in its diversified compartments as manifested to us by the countless centers of the brain, and leads us without a peradventure to the conviction that by exciting one set of centers and inhibiting other centers, we may induce hypnosis, and by a parity of reasoning it is easy to convince ourselves that by the same token we have the *open sesame* to the understanding of the whole physiology of the encephalon, and its pathology as well.

TUMORS OF THE MAMMARY GLAND.

Read in the Section on Surgery and Anatomy, at the Forty-sixth Annual Meeting of the American Medical Association, at Baltimore, Md., May 7-10, 1895.

BY W. L. RODMAN, A.M., M.D.

PROFESSOR OF SURGERY AND CLINICAL SURGERY IN THE KENTUCKY SCHOOL OF MEDICINE, ETC., LOUISVILLE, KY.

There is no surgical subject the literature of which is less satisfactory, than that of mammary tumors. This is not due to a lack of writers, but rather to their contradictory positions.

In the first place the nomenclature of the subject could not well be in a worse condition. The American, English, German and French schools each has a classification peculiar to itself. The general reader finds himself hopelessly at sea, in endeavoring to reconcile the teachings of standard authors.

The only true classification—one based upon accurate pathology—has not been followed. Growths considered by some as benign, are by others treated as malignant. This confusion, noticeable as it is with malignant neoplasms, is much greater with benign affections. Take, for instance, the most common of benign tumors, and we find it described under the same name by only two recognized authors. It has at least a dozen names. The fibroma of Gross, Labbe and Coyne is the adenoma of Broca, the adenofibroma of Billroth, the fibro-adenoma of Cornil and Ranvier, the mammary glandular tumor of Paget, the chronic mammary tumor of Cooper, the pancreatic sarcoma of Abernethy and the lobular imperfect hypertrophy of Birkett. Many of the English writers also call this growth "adenocele." Could confusion be greater?

Further, villous papillomata which are innocent and never repullulate when properly removed, are by some authors described as tubular or duct cancers. They are admittedly somewhat similar pathologically, yet withal very different in a most vital feature. The duct is a perfect capsule to the papilloma, beyond which it never extends. The cancer defies the barrier and proliferates beyond its confines. Therefore one must be somewhat arbitrary and follow the author whose position seems to him best chosen.

Operators are in a large measure responsible for this confusion. The gross or macroscopic appearance of a tumor after removal is of great value, and is at times scarcely secondary in importance to a careful microscopic examination. I have seen many distinguished operators who never touched a tumor after its excision, but felt that their duty was performed when an assistant was told to take a part of it to a microscopist. The latter, however experienced, may get a very incorrect idea of the growth from an isolated portion of it removed at random. The most careful microscopist will often properly refuse an opinion when asked for one under such circumstances. He is entitled to the entire growth, with a full clinical history of the case. The presence or absence of a capsule, secondary degenerations, cystic transformations, whether it was central or peripheral, growing from the upper or lower surface, adherent or not to the surrounding tissues, are all facts rightly his, before a growth is prepared for the microscope. Should one do this in all cases, I am sure he will have less cause to dissent from the final verdict of the microscope, which must be our court of last resort.

According to a recent and excellent authority, Mr. Raymond Johnson, of London, the general opinion that breast tumors originally benign are prone to degenerate into malignancy, is incorrect, and can not be proven. In a series of lectures delivered at the Royal College of Surgeons, of England, in June, 1894, Mr. Johnson devotes considerable time to combating this well-nigh universally accepted view. He asserts that adenoma, which is by most authors supposed to be exceedingly liable to cancerous change, never does so, and says that no one has ever reported a case where an encapsulated tumor as the adenoma, has been seen bursting through its capsule and infiltrating the surrounding tissues. Until he has seen such specimens and examined them macroscopically and microscopically, he can not believe they are of common occurrence, if indeed they exist at all.

ETIOLOGY.

Notwithstanding their frequency, it is surprising how little is known of the causes of mammary neoplasms. S. W. Gross showed very clearly by a most patient and careful analysis of a large number of cases, that many opinions more or less well fixed in the minds of professional men were absolutely untenable. The social condition—hitherto supposed to play such an important rôle in the etiology of breast tumors—is absolutely without influence. Single and married, sterile and fruitful women are equally liable. Trauma plays the same rôle in the production of innocent and malignant growths—11.94 per cent. of the former, 11.70 per cent. of the latter. Disordered menstruation, hysteria, etc., cut no figure whatever. Heredity perhaps has an influence, but it is slight. Gross concludes that heredity can not be shown in non-carcinomatous growths, while it exists in one case out of every nine carcinomas. Eczema and psoriasis of the nipple and areola—usually spoken of as "Paget's disease"—were thought by Gross and others to cause both innocent and malignant growths. Recent investigations by Raymond Johnson, W. Roger Williams and others on villous

or duct cancer, are in line with the teaching of Thin who some years ago stated it as his belief that the so-called Paget's disease instead of being the cause of tumors was simply the result of tubular growths, the discharges from which caused the irritation and angry condition of the areola. This position, I am satisfied, is undeniably correct in the vast majority of instances.

Puerperal mastitis seems to leave lumps in the breast, which are followed by innocent neoplasms in less than 2 per cent. of all cases, while malignant tumors follow such conditions in rather more than 8 per cent. In many instances, however, the neoplasm does not follow for five, ten, twenty or even thirty years—about fifteen on an average. We may, therefore, with propriety doubt that such influence is great.

RACE.

I have not seen it stated in any work on mammary tumors that race exerts any influence. That it does, so far as malignant disease is concerned, I have no doubt. The negro is relatively immune from cancer. In twenty years' experience passed in the South, where the negro population is as 1 to 5, I have encountered malignant disease in the breast of the full-blooded African but once. During ten years of this time I have been constantly on the staff of the Louisville City Hospital, and in charge of the surgical clinic of my college, where our patients are largely negroes. Prof. J. M. Holloway, of Louisville a surgeon of forty years' experience, doing for twenty years a very large business, tells me that after a careful examination of his case-book, he has seen 170 breast cancers, and only 2 were in negroes.

At a recent meeting of a Louisville society I asked the Fellows present to speak upon this subject. Two members, each of whom have been in practice over thirty years, had never encountered malignant disease in the breast of the negro. All agreed that it was rare.

Young, also Livingstone, in his book of travels, and Walshe in his famous work on cancer as geographically distributed, emphasize the relative immunity of the African from malignant disease in general. Billings has recently made the same observation. Therefore, we should not be surprised at the difference we find in the relative frequency of breast tumors in the two races. I believe, however, that the same disproportion will not be found to exist in other regions of the body.

In the white woman, cancer of the breast and the uterus is on a parity, for, according to Herbert Snow, "of 467 cases of cancer at the London Cancer Hospital in one year, 115 suffered from malignant disease of the breast and exactly the same number from that of the uterus." Each organ represents about 25 per cent. of all cancers in women. So that one-half the cases of cancer in females affect the breast and uterus. Other regions of the body furnish the same number of cancers in the male and female; therefore the disease is twice as common in the latter.

Diagnosis.—The superficial situation of the mammary glands, the frequency with which they are affected with neoplasms of a destructive nature, would presuppose a more exact knowledge of their diagnosis than we can truthfully lay claim to. That mistakes in diagnosis are common, more so than with tumors of any other region of the body, few, I think, will deny. There are, however, trustworthy data, in so far as they go.

The age of the patient is a valuable guide. In general, young subjects have benign growths and sarcoma, whereas middle and advanced life are most obnoxious to cancer. Malignant growths are by far more common than benign, the proportion being rather more than 10 to 1. During the rudimentary state of the gland, benign growths (usually fibroma) may be encountered. Gross in an analysis of 777 cases taken largely from reports of German surgeons, did not find sarcoma under the fourteenth year. I have seen journal reports of cases in children under 10; one in an infant. The average age of patients with benign growths is rather under 30. They constitute less than 10 per cent. of mammary tumors (9.5).

Sarcomas occur from the beginning of menstruation to the seventh decade of life, the average age being 35 years. They constitute less than 9 per cent. of breast neoplasms.

Cancer has been seen at 21, by Henry, though it is rare under 30. The average age for carcinoma has been placed by Gross at 48. It has been seen after 90.

Villous or duct cancers, according to Roger Williams occur at an average at 53½ years. Cancer claims about 82 per cent. of all mammary neoplasms. Undoubtedly the most valuable guide in diagnosis, is the presence or absence of enlarged lymphatic glands.

For convenience of comparison, we may group sarcomas with benign growths, as the former are little if any more liable to cause enlargement of the neighboring lymphatic glands. Non-carcinomatous tumors cause enlargement of the axillary glands in less than 3 per cent. of all cases. The supra-clavicular glands are never enlarged. The enlargement of the axillary glands is largely due to irritation, and not the result of infection. They are soft and not adherent to the peri-glandular tissues. Carcinoma, *per contra*, practically always causes enlargement of the neighboring lymphatic glands, first the axillary, then the supra-clavicular.

It may be noted here that villous cancer caused enlargement of the axillary lymphatic glands in only five out of eighteen cases seen by Roger Williams, due, according to Labbe and Coyne, to a barrier of fibrous tissue between the cancer and the nearest glands.

In 65 per cent. of all cases the axillary glands are enlarged when patients first apply for relief—an important fact to remember when operative measures are undertaken. The glands are hard, manifest a tendency to coalesce, and soon become adherent to adjoining structures.

Of the remaining differential diagnostic points, the location of the tumor and whether it be mobile or not are the most important. Benign affections as a rule are movable, the malignant soon become fixed. Benign growths are usually located in the upper and inner portion of the gland, while malignant disease is more liable to affect the upper and outer quadrant and to be situated behind the areola. The latter circumstance explains the frequency of retraction of the nipple in cancer. Gross found it in 52 per cent. of his cases, a larger proportion than other observers have seen.

Non-carcinomatous tumors cause a displacement of the nipple in less than 6 per cent. of all cases. In the early stage of breast tumors when any diffi-

culty of diagnosis might be expected, such symptoms as pain, enlarged veins, discharge from the nipple, history of trauma and heredity are practically valueless.

To conclude, age, location of the tumor, its mobility, and the condition of the axillary glands are our surest guides. Doubt as to whether or not a growth is solid or cystic will be settled by aspiration. True cysts are rare, making less than 2 per cent. of all cases. Cystoid degeneration of solid growths is common, particularly so in non-carcinomatous tumors. Cystic degeneration of the acinous varieties of cancer rarely occurs, though it is more frequent in the tubular or villous growths.

An aseptic incision into the tumor for diagnostic purposes is proper, and is advised by Bull of New York, Keen of Philadelphia, and others.

Treatment.—Little need be said concerning the treatment of benign tumors, solid and cystic, as all are practically agreed. They should be removed along with their capsule, and in such a way as to leave the nipple when it is practicable. When a tendency to rapid growth is shown, and the patient is past 40, it is far safer to excise the entire gland, for it should always be remembered that mistakes in diagnosis are common, and in patients of this age the chances are 13 to 1 in favor of malignant disease.

How shall we treat malignant disease, and is it curable? Reports from many distinguished sources during the past year especially, leave little room for doubt that one of the brightest chapters in the history of operative treatment for malignant disease is now being written. The day for pessimism has passed. American operators have so far secured the best results, with the Germans a close second. The English, with whom the complete or radical operation as practiced to-day originated have, strange to say, fallen behind. This is, no doubt, due to the fact that some of their best authorities as Butlin, Treves, Sutton and others have thrown all their influence against radical measures. The writings of these gentlemen have evidently to a great extent fashioned professional sentiment throughout Great Britain.

In 1867 Moore, of London, first advocated the complete operation as it is understood at the present time. He advised thorough extirpation of the entire mamma, removal of the pectoral fascia in all instances, supplemented by cleaning out the axilla. His teaching passed unheeded for a time, but was soon taken up by Banks in England, many of the Germans and Austrians, notably Volkmann, Billroth, Langenbeck, Knüster and others. S. W. Gross, of Philadelphia, was quick to see the possibilities of the radical operation, and was its first champion in this country. I was an interne in Jefferson Hospital during the time he was writing his "Monograph on Breast Tumors," and assisted him in many of the complete operations reported in this work. No one who ever heard him lecture, or saw him operate upon breast cancer, can forget his intense earnestness born of deep conviction, at a time when all of his colleagues —his great father included—to draw it mildly, looked upon him as a misguided optimist. In the lecture room, wards of the hospital, and his own private office, I have heard him say a hundred times that no surgeon however experienced, could rightly affirm that the axillary lymphatic glands were not enlarged, until the axilla was opened and inspected from base to apex. This is now accepted of all men. All honor to this brainy, aggressive, prescient surgeon!

Excellent as were Gross' results—12 to 15 per cent. of cures—they have been improved upon by subsequent workers in this line, aided by more modern methods. The present occupant of his chair at Jefferson, Prof. W. W. Keen, is doing work equal to the best. Weir and Dennis, of New York, each report a series of cases, 125 and 33 respectively, the former getting in round numbers 20 per cent., the latter 25 per cent. of cures. By far the most valuable, instructive and encouraging report up to date is that of Bull, of New York, published in the *Medical Record* of August, 1894. No one has followed up cases so methodically as he has done. Only 3 out of 118 cases operated upon between 1880 and 1894 remain unaccounted for; 115 were followed to their death or to Jan. 1, 1894, when their condition is fully set forth. Excluding cases operated upon since 1891, as a three year limit has been generally agreed upon before a cure can be reasonably claimed, there are left seventy-five cases submitted to the complete operation as defined by Moore. Of this number four died from the operation, two from erysipelas, one from sepsis and one from pneumonia; fifty succumbed to recurrences or metastases; two are living with recurrence; four died of intercurrent affections after having passed the limit, and the remaining sixteen are in perfect health. Counting the four dying of intercurrent disease after passing the limit, there are twenty cures or 26.6 per cent., the best showing yet made by any surgeon. Deducting five cases which should not have been included and Bull has nearly 29 per cent. of cures. Of the sixteen still living it may be said that there has been an average of six years since operation—twice the usual limit.

It is also of great interest to note that 40 per cent. of Bull's cured cases had cancerous involvement of the axillary lymph glands, as demonstrated by microscopic examination. Therefore, a complete operation is able to cure, even after lymphatic involvement has occurred.

This series further shows that where no lymphatic glands are involved, 54 per cent. are permanently cured. This sounds strangely enough, I know, to those who remember that such men as Sands of New York, and Agnew of Philadelphia, each said at the close of an illustrious career in surgery that he had seen no successful case of operation for breast cancer.

Results are getting better, year by year, month by month, almost day by day. The trend is in the direction of a still more complete operation, and the value of it is shown by a brilliant series of fifty cases recently reported by Halsted of Johns Hopkins Hospital. In every case he removes the larger portion of both pectoralis major and minor muscles. He also removes the axillary and supra-clavicular glands in all cases. This seems necessary in a certain per cent. of cases to get beyond the invisible zone.

Volkmann's observation, that infiltration of the muscles sometimes occurs, has been verified by Heidenhain after the most careful investigation. For a time, fortunately, the pectoral fascia arrests the march of cancer.

Willy Meyer, of New York, has proposed an operation even more radical than Halsted's, as he removes all of the great and lesser pectoral muscles from origin to insertion. Both Halsted and Meyer assert that such radical measures add nothing to the dan-

ger of the operation and, further, do not leave the patient with an arm materially less useful. Halsted has reported seventy-six such operations without a death, and with only 6 per cent. of local recurrences. It is too soon to estimate his percentage of cures.

If statistics prove one thing, it is that partial operations should no longer be practiced. They do little good and are almost as dangerous as the complete method—where the axilla is invaded and all glands and fat removed—vessels and nerves only being left behind. The space of Mohrenheim—between the upper border of the tendon of the small pectoral muscle and clavicle—is to be systematically inspected and cleared in every case. If the two pectoral muscles are not removed, the space between them must be carefully examined for outlying gland tissue and fat.

What is the mortality of such operations? Taking the reports of six American operators recently given to us and we find that in 630 operations for cancer there have been 6 deaths. In nearly all of these cases the complete operation was practiced. W. W. Keen reports 200 cases with 1 death. Bull 118 with 4 deaths and Dennis 71 with a single death. Weir, Halsted and Powers have reported series of 125, 76 and 50 respectively, without a death. Curtis gives the average mortality of the leading German surgeons as 6 per cent. though many operations done before modern methods were practiced are included.

The English (Butlin, Treves, Williams and others) place the mortality as high as 10 per cent. This can only be reconciled with the rather less than 1 per cent. of six American surgeons, on the score of many operations being included which were done at a time when breast excision was properly ranked as a dangerous measure. Erysipelas, sepsis, secondary hemorrhage, etc., claimed their victims by hundreds. The matchless Billroth years ago admitted a death rate in his own practice of 23 per cent.

We can only contemplate such figures now with feelings akin to horror—as an aseptic operation in skillful hands is to-day practically devoid of danger. We conclude with the following summary:

1. Mammary cancer, submitted to operation before infection of the axillary glands, should promise 50 per cent of cures.

2. Although the axillary glands seem not to be involved, the axilla should be opened in every case and thoroughly cleared of glands and fat.

3. Infected axillary glands are of bad prognostic import, but a radical operation which removes them and at the same time the supra-clavicular glands may cure 11 per cent. of such cases.

4. The complete operation should be practiced in every case of malignant disease.

5. The mortality following the complete method is doubtless somewhat greater than it is in partial operations. The difference in results, however, is so conspicuous when the question of "cure" is considered, that the radical operation is the only one to be countenanced.

6. Statistics gathered many years ago are as valueless as those made use of in estimating the mortality after amputation of the extremities.

7. The mortality will probably be not more than 2 or 3 per cent. with the average operator. We have shown it to be less than 1 per cent. in over 600 cases operated upon by six American surgeons.

EXTIRPATION AND COLOTOMY IN CASES OF CANCER OF THE RECTUM.

Read in the Section on Surgery and Anatomy, at the Forty-sixth Annual Meeting of the American Medical Association, at Baltimore, Md., May 7-10, 1895.

BY LEWIS H. ADLER JR., M.D.

PROFESSOR OF DISEASES OF THE RECTUM, PHILADELPHIA POLYCLINIC AND POST-GRADUATE SCHOOL; SURGEON TO THE CHARITY HOSPITAL, AND TO THE OUT-PATIENT DEPARTMENT OF THE EPISCOPAL HOSPITAL, PHILADELPHIA, PA.

Colotomy and extirpation are two recognized procedures for the relief of malignant disease of the rectum. Both operations offer to the patient a chance of prolonging life; and, in addition, extirpation holds out the possibility of effecting a radical cure, in selected cases.

The choice between these two methods is a question of uncertainty only in a relatively small group of cases, and extirpation is not to be considered in the majority of instances, for the reason that the disease is usually an incurable malady, and by virtue of its concealed position within the rectum its presence is not revealed or even suspected until the growth has existed for some time. Furthermore, it is rare for cancer of the rectum in its incipiency, to manifest itself by any symptom pointing to a lesion within the bowel. This is a familiar observation to all surgeons.

It frequently happens that a patient comes to us complaining of some slight diarrhea or other mild rectal trouble, and an examination unexpectedly reveals the fact that cancer is present to such an extent that it is obvious the neoplasm has existed for a considerable period. Consequently, its complete removal is often rendered impossible. Again, the patient's vitality is such that so grave an operation and one requiring the length of time for its performance, as an excision, is contra-indicated.

In arguing thus, I would not convey an impression that I am opposed to extirpation for malignant disease of the rectum in suitable cases; on the contrary, I firmly believe it to be a perfectly justifiable operation when the growth is circumscribed and confined to the lower five or six inches of the bowel; provided, however, that the tumor does not involve all the coats of the intestine, that it has not attacked the visceræ which are intimately associated with the anterior wall of the rectum, that it has not invaded the pelvic glands, or, by metastasis any of the other organs of the body, and finally, that its growth be not rapid or have a tendency to spread widely. From these considerations, it naturally follows that the number of patients who can be benefited by excision of the disease is comparatively small.

Colotomy, on the other hand, is indicated in a large number of instances, where it is quite impracticable to attempt an excision. The advantages of the operation lie partly in the relief it affords to symptoms and partly as a means of retarding the growth of the neoplasm.

To indicate the relief afforded such patients by a colotomy, I can not do better than quote the opinion of Kelsey on this subject as, expressed in the fourth edition of his work: "Diseases of the Rectum and Anus."[1]

This authority is not only a strong advocate of the operation, but is also in a position to judge of its merits by reason of his large experience. He thus states his views: "As to the benefits arising from the

[1] Pp. 409 and 410.

operation, too much can scarcely be said. That it prolongs life by the relief of pain, the preventing of obstruction, and retarding the growth of cancerous disease is beyond question. That it substitutes in many cases a painless death for one of great agony is indisputable. The idea that it is as well to let a patient die as to subject him to a colotomy has no supporters among surgeons who have had any experience with these cases. Indeed, I think that the practitioner who to-day sat by and allowed a patient to die of obstruction because of any sentiment against this procedure would hardly be held blameless. I can only say that, after trying every other means of treatment and being obliged to admit the fruitlessness of them all, I have come, with most others, to admit the great benefits of colotomy, and *have never performed it in any case in which either the patient or myself has afterward regretted it.*" (Italics mine.)

In another article,[2] Kelsey mentions even more minutely the advantages of this operation, as follows: "Colotomy, especially inguinal colotomy, relieves pain; does away with the constant tenesmus and discharges from the rectum, which by their exhausting effects are the immediate cause of death; delays the development of the disease by preventing the straining and congestion of defecation; prevents absolutely the complication of intestinal obstruction, which is another cause of death; enables the patient to sleep, eat and gain flesh, and often makes him think himself cured in spite of the plainest prognosis to the contrary. Instead of passing his days and nights upon the commode, wearing out his life in the effort to free the bowel from its irritation, he has one or perhaps two solid fecal evacuations from the groin in twenty-four hours."

In conclusion, I would like to allude to one more topic, to-wit: the choice of sites of opening the colon. I mention this subject with the sole purpose of eliciting the present views of the members of this society. My own belief is that the inguinal region is to be preferred in the majority of cases. Its advantages over the lumbar operation are, to my mind:

1. The smaller incision and lesser depth of the wound requisite to reach the colon, and the minimum amount of disturbance of the structures overlying the seat of operation;

2. The greater facility offered for the exploration of the abdomen, when such a procedure is required;

3. The better position for safe anesthesia, during the operation;

4. The comparative ease with which the colon may be identified in this position, and the little difficulty experienced in fixing the bowel to the skin without undue tension on the stitches;

5. The greater readiness with which a good spur may be formed;

6. The convenience to the patient of the site, for purposes of cleanliness and for the adjustment of pads; and

7. The recent statistics seem to indicate that it is the less dangerous operation.

1610 Arch Street.

Influenza and Life Insurance.—At a meeting of one of the large English insurance companies it was shown that more than six hundred thousand dollars had been paid out for deaths due to influenza. The report of the Secretary showed that this disease has cost the insurance companies more in the last two years than in the previous forty-three years.

[2] New York Medical Journal for November, 1892.

IS TOTAL EXTIRPATION OF THE RECTUM EVER JUSTIFIABLE?

Read in the Section on Surgery and Anatomy, at the Forty-sixth Annual Meeting of the American Medical Association, at Baltimore, Md., May 7–10, 1895.

BY JOSEPH M. MATHEWS, M.D.

PROFESSOR OF SURGERY KENTUCKY SCHOOL OF MEDICINE, ETC., LOUISVILLE, KY.

In giving the above title to my paper the fact is indicated, at least, that such a procedure is advised, and I may add, is strongly recommended by many. With true deference to all such, I shall raise the question as to its justification, and will try to adduce sufficient reasons for putting the question in the attitude of being mooted. It can not be gainsaid that some, if not many surgical operations, are done with but little to recommend them. In this age of progress in surgery, I opine that some of us are led astray into doing operations as much to test the efficacy, methods, etc., of others, and to put ourselves in line, as to accomplish that which should be the surgeon's chief aim, viz., to relieve the patient. It can not be denied either that there is what might be properly called a *rage* in surgical work, and it behooves us as surgeons to occasionally take a breathing spell, and look backward over the field, if not over the dead. I am quite willing to admit that conservatism in surgery may oftentimes mean early and complete operations, but it must be admitted that he is the best surgeon who has studied well his subject from all standpoints and is ready for any emergency.

To operate or not to operate, is often the question, and much responsibility rests on the decision. If the eager, ambitious, aspiring young surgeon could have in his time of need, the advice of his cooler-headed elder brother, who has met with many a danger, what a blessing! We are much given in this country to copying after our European brethren, even to their vagaries, in both medicine and surgery. New instruments, new operations, new books, new everything, is the cry with us, and we don't stop to think whether they are any improvement over the old or not. Oftentimes, too, the *original* operation is far preferable to the so-called modification of the same. With these remarks as a preface, I desire to call your attention to the question propounded in the title of my paper, Is total extirpation of the rectum ever justifiable? Very naturally you will say, Why, I thought that question had been settled, and that most authors recommended such an operation under certain conditions. You might add in truth that Mr. Kraské had designed his operation especially for this purpose. To both propositions I answer, quite true. But in the same breath I would beg to ask, Is the operation justifiable? An operation might be designed which would demonstrate the fact conclusively that the liver could be easily and dextrously removed. There are two propositions that I would submit for argument in reasoning against the advisability of the total extirpation of the rectum for any cause:

1. Is the patient ever cured of the trouble operated for, by said operation?

2. Is the patient ever materially benefited or relieved by the operation?

I beg to take the negative to both propositions.

1. I take it for granted, that the only cause for which it could be said that total extirpation of the rectum should be practiced is malignancy. I use the term here as synonymous with cancer. We note,

then, the characteristic features of malignancy as: the disposition to grow, to ulcerate, to infiltrate and to propagate. It is an aphorism well known in surgery, in cases of cancer, "to operate early if you desire success." All teachers too, know how assiduously they teach the student to do a *thorough* operation, and "cut wide of the mark," whenever dealing with this character of tumor." For instance, when removing malignant growths of the breast, how important it is, we say, to remove the glands in the axilla also. Again, how insidiously does a malignant tumor invade the system, and establish a cachexia. How difficult to tell the dividing line between a local condition and a general infection. Add to this the suspicion always that the tumor has likely propagated.

These questions are difficult of solution, even when the growth is situated in or upon the external parts. How much more difficult when located in an obscure part of the anatomy as is the rectum. Cancers found here are so stealthy in their growth and invasion of tissue that oftentimes the whole rectum is blocked by the mass, when only symptoms of constipation and some reflex pain are complained of. The tissues surrounding this portion of the gut are soft and easy of invasion; the blood circulation is great and the lymphatics are freely distributed. The contiguous parts are vital ones; the bladder, the peritoneum, the prostate gland, the vagina, in the female, are all easy of invasion. By the time that a growth in the rectum has assumed sufficient proportions to be diagnosticated as cancer, many of these parts are already invaded by the process of infiltration. To dissect and draw down the rectum in the normal state may be comparatively an easy matter, but to completely dissect out a rectum that is so pathologically changed, as in a cancerous growth, is nearly a physical impossibility. Beside the difficulty of accomplishing its free dissection, it is one of the bloodiest operations that I have ever attempted. Admit that it can be successfully done, I would ask what amount of good accrues to the patient? Infiltration is clearly proved by the amount of adhesion that is witnessed. Can we, now, "cut wide of the mark?" I believe that every surgeon present will concur in the opinion that to make the operation complete, all glands in the inguinal region should be removed. But what about the infiltrated tissue left in and around the rectum? It does not require very much dissection to draw down the natural gut. Sever its connection at the anus, and it can be pulled down. But it requires a long, tedious, and free dissection to remove the cancerous rectum. What, I again say, shall we do with the injunction to "cut wide of the mark?" It can not be done without invading with the knife the vital parts mentioned, nay not then. Then of what use is the operation? A bloody procedure, rectum gone, and infiltrated tissue left. Would any sensible surgeon dissect out a cancer in the breast and cover the wound with infiltrated flaps? Why, then, should a surgeon dissect out a cancer of the rectum and leave infiltrated tissue? How can he prevent it, or how is he to know that infiltration into the adjacent tissues has not taken place?

It is nearly self-evident from the clinical history of all these cases and from the pathologic changes, that such condition does exist. A cancer of the rectum in its incipiency is very difficult to detect, and is rarely seen by the surgeon in this stage. If it invades the anal orifice first, this rule does not hold good, but no surgeon is likely to remove the entire rectum for a growth at the anus. It must be admitted that it is in the incipient stage of cancer that an operation would be most justifiable. And yet for a nodule along the course of the rectum, any surgeon would hesitate about removing the whole rectum. Then if it is not justifiable to totally extirpate the rectum for a circumscribed or incipient condition of cancer, is it justifiable to remove the same after adhesions, infiltration and a cachexia has been established. When, I would ask, is total extirpation of the rectum justifiable?

All malignant growths affecting the anus first should be removed, if seen at the proper time. If a growth of this kind is situated in the immovable portion of the rectum, and has not extended higher than this, or has not invaded vital structures, the surgeon can remove it, observing the injunction to "cut wide of the mark." But this is a very different thing from removing the entire rectum and that, too, when no promise can be given. I have written this paper after a fair trial of all methods proposed for the total extirpation of the rectum. But as I have usually seen these patients go on from bad to worse, I must confess that some remorse of conscience has overtaken me when the question stared me in the face, why I did so formidable an operation, when in truth there was no good surgical reason for doing it. This is an unfortunate class of patients, the most unfortunate of all, and they do indeed catch at straws. They need our fullest sympathy, and nothing less than a firm conviction that we can either cure, or materially benefit them, should lead the surgeon to do so radical an operation as the total extirpation of the rectum. Can we promise either? I think not.

THE CAPACITY OF MEDICINE.

The Doctorate Address to the Graduating Class of the College of Physicians and Surgeons, Chicago, April 2, 1895.

BY WM. ALLEN PUSEY, A.M., M.D.

PROFESSOR OF DERMATOLOGY, COLLEGE OF PHYSICIANS AND SURGEONS. CHICAGO.

The man is fortunate who has a high conception of his life work. In this respect, at least as regards the humanitarian aims of his profession, the doctor is usually fortunate. I believe it is the rule that the doctor has a conscious pride in the high purposes of his profession; I believe it can be said, without exaggeration and without straining the truth, that the doctor usually approaches his work with a realizing sense of his duties and of his heavy responsibilities. Often—oftener I believe than most men—he is animated by lofty ideals and sustained in a career of heavy responsibilities and, perhaps, unrewarded endeavors by the approval of his own conscience for doing, as well as in him lies, the work that comes to his hands. I believe, too, it can be said with truth, that he does not usually have to depend upon the approval of his own conscience for his sole reward. For the world at large is usually ready to applaud him for his good work and to praise him for his charity and his humanity. In fact, excepting here and there a soured pessimist or a fanatical anti-vivisectionist, who would cast upon us all the anathemas of the damned because we sometimes kill dogs and guinea pigs in order to save babies—excepting these, I say, I believe the world in general gives us credit for high purposes and for untiring efforts in behalf

of mankind. Indeed, one sometimes fears that they emphasize the goodness of our intentions and our charity and benevolence, because they feel that in these they have the safest grounds upon which to praise us; just as at times one calls a man a good man, when little else can be said in his favor. For it is undeniable that medicine has to stand a good deal of patronizing and condescension, particularly from people who imagine themselves progressive and from those workers in other departments of science, who, knowing only their own fields, imagine that they possess all there is of scientific merit. Every once in a while some innocent seeker after truth wants to know, "If medicine is a science?" We are reminded of the advancement of other arts and sciences in this century, and are asked in a hopeless sort of way if medicine has anything to offer commensurate with these. We are not allowed to forget that medical opinions have changed from time to time; that the doctrines of one period have given way to different doctrines of another; and that even now doctors disagree; just as if these things were the unique characteristics of the medical profession. And the intimations are not uncommon that medicine, while perhaps advanced in theoretical knowledge, is behind in practical realization; that, to paraphrase Montaigne, it has always known its Galen, but does not yet know its patients; that it has always been full of fruitless promises, but has given to the world little of useful results or of stable truths of practical value.

This mild and perhaps well-meaning skepticism is widespread, and finds expression in various forms. It is not enough to say in reply that the most cursory consideration demonstrates the falsity of such statements; or that scientific medicine is too strong to be affected by the impressions of the half-educated or the judgments of the inexpert. The man, who is in touch with his surroundings, can not be insensitive to such influences, and I question if the doctor is fortunate in his complacency who has not at some time asked himself how much justice there is in these imputations. Indeed, the doctor who starts out with enthusiasm for his profession and with high conceptions of its usefulness will probably, for his own satisfaction, do well to ask himself the questions to which such aspersions give rise. And to ask ourselves these questions now: What, really, is the position of medicine? Can medicine pretend to stand on an equal footing with the other physical sciences? Is it worthy of a place beside chemistry, or physics, or mechanics, as a science, or beside electrical, or mechanical, or civil engineering, or manufacturing chemistry as a technical profession? Or, to put it practically, can medicine show any solid contributions to useful knowledge in the last hundred years comparable to the much vaunted contributions of steam power, of electricity, or of mechanical invention? Has medicine shown in the past, or does it show now, an ability to meet its obligations and to fulfill the demands upon it equal to that of the other arts and sciences? And now on the verge of this twentieth century, can medicine pretend to prospects of usefulness and development as promising as those of any other of the physical sciences?

These questions suggest themselves with more or less distinctness to all of us at times, and it may not, therefore, be unprofitable to consider for a few minutes the position and the capacity of medicine as shown in its course in the century now drawing to a close.

The nineteenth century, in respect to the progress of the physical sciences and the development of useful knowledge and inventions, is a period marvelous to contemplate. When one reviews in his mind what had been done in these departments of knowledge before 1800, and what has been done since, he finds it difficult to characterize the period without apparently indulging in the grossest hyperbole and exaggeration. The wildest imagination of a hundred years ago could not have faintly conceived the progress which this century would witness. This century has, in fact, reaped the harvest of the Renaissance. Perhaps it would put it more accurately to say that this century represents the entrance of the physical sciences into their period of adult vigor. All that had gone before, since the revival of learning—and practically nothing before the revival of learning is of any consequence in the physical sciences—represented the period of preparation.

At the end of the last century the times were ripe for advancement. Awe of tradition, even respect for the established order of things had vanished. Revolution was in the air. The spirit of unrest and of independence, of rebellion against privileged classes and arbitrary authority that had begun with Luther in religion and Galileo in science, that had driven the Puritan pilgrims to America, and had produced a Cromwell in England, had finally resulted in an American rebellion, and the establishment of a government by the people; and had reached its climax in the awful upheaval of riot and license of the French Revolution. The established order of things had lost its sanctity. Men were ready to prove all things, but to take nothing on faith or on tradition. And that is the true spirit of science. And, moreover, science and art suited the spirit of the times, in that they represented an aristocracy of intellect in contra-distinction to an aristocracy of birth. The times were, in fact, as ripe for a revolution in knowledge as they were for the civil and social revolutions that were occurring.

And yet one at that time, looking for indications of the future, would have found little positive material on which to foretell the advancement of this century. Of the pure physical sciences, astronomy was the only one that had reached a condition of anything like completeness. Of the broad generalizations that are at the same time the glory and the very foundation of physical science none, except Newton's law of gravitation had been worked out. Laplace was just indicating the fascinating but uncertain hypothesis of the nebular origin of the solar system. Dalton had not yet enunciated the atomic theory, upon which modern chemistry has been built. Schwann had not yet demonstrated the cellular structure of vegetable and animal tissues, which is the very basis of modern biology. And more than half the century was to pass before Darwin promulgated the doctrine of natural selection, which has given to the world a new conception of the order of development of living things. Perhaps the most important generalization of physical science, Joule's law of the conservation of energy, which permeates every conception of modern science, was to wait for its enunciation until after the middle of this century. Newton's corpuscular theory of light and the conception of heat as due to a vague "caloric" still prevailed. These facts in their bare statement convey no im-

pression of their momentous importance. Unverifiable hypotheses, as some of them are, they are nevertheless the very foundation of modern science; and the times when they did not exist are times in science that you and I can only realize by a strong effort of the imagination.

And if pure physical science, as we know it to-day, was thus undeveloped the mechanical arts and the practical applications of science to the uses of civilization, which are the peculiar characteristics of our times, were even more so. Watt had but recently made the steam engine into a practical servant of man and its usefulness was not yet established. The first steamboat had not been built; and as for railroads, the memories of men now living take them back to the time when Stephenson's "Rocket" demonstrated the practicability of a steam locomotive. The spinning-jenny, the automatic power-loom, the cotton-gin of Eli Whitney—these three fundamental inventions, which have rendered possible the vast textile industries of modern times—had just been invented; and that other boon of laboring women, the sewing machine, was not to come for fifty years. The electric telegraph had hardly been thought of, and its invention was away in the future; and as for the possibilities of submarine telegraphy, of electric lighting, of photography, or of transmitting the human voice by electricity, who could have dreamed them a hundred years ago?

A hundred years ago men were, in fact, as regards mechanical inventions, much in the same place that they had held from the earliest civilization. They cleaned their cotton and carded their wool; they spun and wove and sewed; they harvested and threshed their grain; they lighted their houses; they sent messages, and traveled by land and sea, in practically the same ways that men had done a thousand or two thousand years before. The list in fact could be indefinitely continued. All the inventions of mechanics; all the applications of physical science to the uses of man, that have made the forces of nature man's servants, that have made intelligence dominant over force, that have welded the civilized world into a sentient whole, with community of interests and community of knowledge; all these, with few exceptions, are the inventions of this century. Could anything be more marvelous than this progress? If medicine had not kept pace with the other departments of physical science in this century would it be a reproach, considering the infinitely delicate and confusing problems which biology and pathology and medicine proper have to solve? But has that happened? Has medicine been outstripped by the other sciences in the nineteenth century? Can medicine, in fact, stand in a free and open comparison of this sort, or must it enter such a contest with excuses and apologies, and with such humility as befits the poor relation of a prosperous family? It requires, perhaps, a reckless degree of temerity to attempt such a question, but let us, as best we can, consider it for a little while.

At the end of the last century, medicine was able to give a good account of itself. The pressing necessity of getting relief from pain and disease had started medicine on a career of usefulness before most of the physical sciences were born. It could point to a history of investigation and of development along the best lines of knowledge of its times, as old as the revival of learning. Beginning with pioneers in scientific research like Vesalius and Ambrose Parè and Harvey and Sydenham it had come down through a line of great observers to such masters as Petit and Bichat and the Hunters and Percival Pott and Jenner. Through the labors of a long list of such men, medicine could fairly claim—in actual achievement and in positive knowledge—a position among the physical sciences as good as any. Gross anatomy, after infinite work, had been brought to a state of practical completeness. Surgery, at its beginning an appendage of anatomy, had attained a degree of skill in which it could claim to meet the reasonable expectations of its times. Operative surgery was highly developed. The great operations have come later, but the ordinary surgical procedures —amputations, the treatment of fractures, the ligation of arteries, and like surgical operations were carried out with intelligence and technical skill. And better than that, men like John Hunter and Pott and Desault and John Bell, were studying the pathologic processes at the bottom of surgical conditions, and were developing a practice of surgery founded upon principles—were indicating the way to a rational practice of surgery, in which surgical procedures would be carried out in the light of a correct understanding of underlying pathologic conditions. In medicine proper the phenomena of disease had been accurately observed and recorded; systemization and classification on a clinical basis had been well done; and diseases had been studied carefully and philosophically for two centuries. Most of the diseases which we recognize now as clinical entities had been differentiated and well described; and the best lines of their management in the light of existing knowledge had been carefully considered. Pathology had been founded by Morgagni, and physiology had made a correct start under Haller in the middle of the eighteenth century.

But the enumeration can not be carried much further. The science of medicine as we know it now, built upon accurate data obtained with instruments and by methods of scientific precision, did not exist. Without any adequate conception of the functions of the various organs; without knowledge of the microscopic anatomy of diseased tissues; without accurate instruments for the investigation of the various elements of disease, medicine, as we know it now, could not exist. What, for instance, could be known of the composition of body-secretions when Priestly had only just discovered oxygen, and fifty years before Liebig and Wöhler had indicated the possibilities of organic chemistry. There was no adequate knowledge of the functions of any of the organs. Schwann had not yet indicated the ultimate cellular structure of animal tissues. Charles Bell had not demonstrated the difference between motor and sensory nerves. Marshall Hall had not discovered the reflex functions of the cord. And Claude Bernard, who was to discover the vasomotor system of nerves, and demonstrate their enormous importance in the nutrition of the tissues, was not yet born. Bichat had just indicated the possibilities of pathologic and normal histology. His superlative genius had carried the subject probably as far as the appliances at his command would allow—for the microscope when Bichat died in 1802 was little better than a toy —but in spite of such geniuses as Bichat and Morgagni before him, a real science of pathology, a thorough-going investigation into the pathologic

factors of any disease, did not exist, could not exist in the very nature of things. As for exact methods of studying disease, there were none. Physical diagnosis had not been discovered. Auenbrugger had discovered percussion as a means of diagnosis fifty years before, but no attention had been paid to it. Laennec had not yet given to the world the method of auscultation or the stethoscope; and as for the other instruments of precision in physical diagnosis, the ophthalmoscope, the laryngoscope, even the duck-bill speculum, and all the minor instruments of diagnosis were as yet unheard of. On the practical side, obstetrics was still in the hands of midwives; and to any one who knows the horrors of ignorant midwifery, could there be a more beneficent example of progress than obstetrics to-day presents? Surgery, in spite of what it had done, was still merely operative; it could only cut off what it could not cure. It was hopelessly handicapped; it could not annul pain during operations, and it could not prevent the ravages of septic infection after operations. In therapeutics all was of necessity dark. The lancet, the purge, and the blister were the main reliances. Excepting opium, all the list of invaluable remedies that soothe the body and relieve pain; all the alkaloids that make exact medication possible are the products of this century. Morphin and chloroform and ether, chloral and the bromides and, of course, all the synthetic anodynes, quinin and the iodids, and salicylic acid, aconite and ergot, corrosive sublimate and carbolic acid are all the products of a later time.

That was the condition of medicine at the end of the last century. It needs no prolonged comment to point out the contrast between medicine then and medicine to-day. In the century that has passed medicine has put itself in such a position that it can attack any question of disease that confronts it with intelligence and with a clear appreciation of the problems that must be solved. All of the fundamental subjects, upon which the existence of rational medicine depends, have been worked out to a condition of nearly satisfactory completeness. The work in none of them is ended, for science is never done, but it can be said, in full view of the facts, that only minor details are to be worked out in the fundamental branches of medicine. Physiology has established with certainty and precision the functions of the body. We have definite and almost complete knowledge of the conditions of the tissues in health and in disease. The processes of pathologic anatomy that underlie diseases have been so thoroughly worked out that only rare and extraordinary conditions remain to be investigated. Physiology, biology, normal and pathologic histology are in a state of practical completeness. The minor problems that they have yet to solve merely indicate the high state of knowledge which these subjects have reached.

Clinical medicine is in a position of definite knowledge. The natural history of disease has been worked out with an accuracy that could not have been foretold. Symptomatology, it may be said without exaggeration, and the differentiation of diseases and groups of diseases have been so sharply and clearly marked out that little more can be expected. But medicine has gone further than this. As is the case in any kind of research the consideration of one set of problems has suggested others; and medicine has solved, and is solving, problems of the most momentous importance to mankind that were beyond the scope of the imagination a hundred years ago. Medicine is obtaining an insight—and not a speculative insight, but a positive insight based on sensible fact—into the intimate causes of disease that no man could have foreseen even fifty years ago. Medicine has gotten to the point where it can say of many important diseases—and the list is rapidly increasing—"This is the cause of the disease; without it the disease can not exist." And it can not only affirm that these are the essential causes, but it can take these essential causes and separate them and study them apart from the diseases which they produce; can accurately work out their life-histories; can carry them through generation after generation of pure cultures; and can then complete the demonstration of their essential causal relationship by reproducing the diseases from their pure principles. What a contrast to the times when men were seeking a universal explanation of disease in plethora, or in congestion, or in any other of the indefinite conceptions with which they juggled in the eighteenth century! And medicine is doing even more than this; it is not only demonstrating these positive causes of disease, but it is separating from them the principles which are the source of their pernicious activity. It is studying these elementary causal principles and already, with positive assurance, is indicating the methods by which their effects can be overcome. There is no science that can show more brilliant investigations than these.

These investigations—in themselves purely scientific—have already borne results of almost inconceivably great practical value. The demonstration by Lister that the formation of pus is caused by the presence of microörganisms and that it can be prevented with mathematical certainty by their exclusion—this demonstration, which resulted in the founding by him of antiseptic surgery, has probably done more for suffering humanity in twenty years than all the uncertain charities of twenty centuries. It has made curative surgery practicable; it has shorn child-bed of its greatest danger; it has banished erysipelas and hospital gangrene from surgical wards; it has relieved the wounds of the battle-field of the dangers and tortures of septic infection—that specter of surgery that probably killed more men in our Civil War than all the bullets of that unhappy time. And Lister's application of bacteriology is but one example of what has been done. Cholera and yellow fever visit us no longer. With proper precautions no man need have typhoid fever—and a hundred years ago typhoid fever had not been described. Lockjaw is under our control; and hydrophobia is robbed of its victims. Proceeding along lines not bacteriologic, medicine has overcome myxedema; it can practically call back to health the obtunded victim of that miserable disease. It can even bring the sporadic cretin to a condition of passing intelligence; and certainly there is no object that would seem to be further beyond the possibility of medical skill than the hideous, drooling, idiotic dwarf, who is the victim of congenital myxedema. And we are not confined to these spectacular instances for illustration of what medicine can do. Medicine can claim to-day to take hold of disease with an accurate knowledge of its natural history and of the underlying conditions that produce it; and on a basis of such knowledge, if it can not do the impossible and prevent what has already occurred, it can claim to manage disease with a clear appreciation

of the conditions to be met and of the contingencies to be provided for. Can any science do more? Could any more be expected?

Medicine, indeed, has reached a position of positive achievement, in which it can court comparison with any of the arts or sciences. And it now stands in a position of promises almost realized that offer to suffering humanity benefits almost beyond comprehension. Huxley in 1887, writing upon the advance of science during the Victorian era, in speaking of medicine, said in words that sound now almost prophetic: "One century since, smallpox was almost as great a scourge (as the plague). Science, though working empirically and almost in the dark, has reduced that evil to relative insignificance. At the present time, science working in the light of clear knowledge, has attacked splenic fever and has beaten it; it is attacking hydrophobia with no mean promise of success; sooner or later it will deal in the same way with diphtheria, typhoid and scarlet fever." The spirit of Huxley's prophecy is being fulfilled as regards many diseases. As regards diphtheria, it looks now as though the prophecy was already fulfilled. There is a good deal of reason to believe now that diphtheria has been conquered. And to the man who has seen the ravages of diphtheria, what greater evidence could there be of the advancement and capacity of medicine than the fact that medicine is to-day in a position in which it can seriously hope to produce a specific for that dreadful disease.

Can there be any question of the capacity of a science that has such achievements as these to offer? Has any other department of knowledge done better? Can electricity even, with its marvelous history show results any more startling? It would seem indeed that the branch of knowledge that has produced modern physiology and biology and pathology, that has discovered the essential causes of tuberculosis and leprosy and cholera, that has given to the world such men as Helmholtz and Virchow and Koch and Joseph Lister and Joseph Leidy—it would seem that that branch of knowledge would need no defense against the charge of being unscientific. It would seem, too, that that same science, that began the century with a discovery that has practically banished smallpox from civilized communities, and is ending the century with discoveries that promise to give us control of some of the worst scourges that afflict mankind; that has given the world immunity from pain under surgical operations, and has produced antiseptic and conservative surgery; that has banished from us scurvy and cholera and yellow fever; and that, on a basis of accurate physiology and pathology, furnishes a rational treatment of disease—it would seem that such a science would no more need a defense against the charge of being impracticable and barren of useful results.

Indeed, I believe we may be pardoned for thinking that medicine is not the poor relation of any scientific family.

103 State Street.

The Chicago Department of Health issued, in June, a circular on the care of infants and young children in hot weather, which can be had in quantity by physicians, at that office. It gives directions regarding food and cleanliness that in many instances will be of value and assistance to the physician and is intended for distribution to the laity.—*Chicago Medical Recorder*, July, 1895.

REPORTS ON TYPHOID FEVER CONTINUED.

NO. VIII.

Read before the Ohio State Medical Society, Columbus, Ohio, May 17, 1895.

BY JOHN ELIOT WOODBRIDGE, M.D.
YOUNGSTOWN, OHIO.

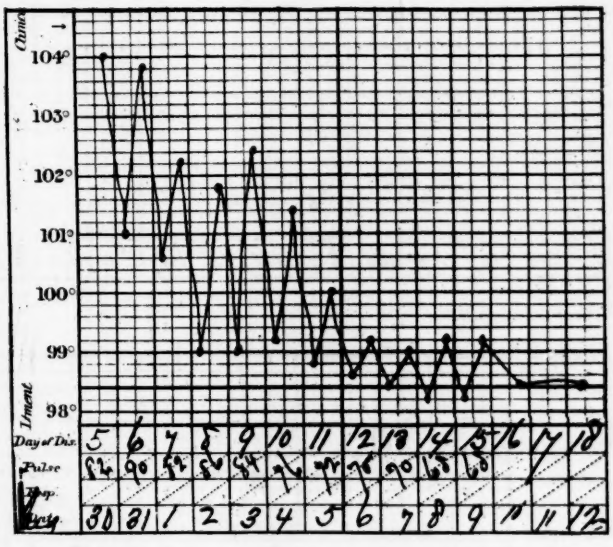
(*Continued from page 59.*)

Dr. E. J. March, of Canton, Ohio, sends me the following letter with the charts, which are marked, "Dr. March's Cases, Harry W., and Sadie J."

"CANTON, OHIO, April 26, 1895.

"*Dr. J. E. Woodbridge,*

"*Dear Doctor:*—An apology is due for my long delay in giving you the reports of some of the cases of typhoid fever which I have treated with the medicine you furnished so kindly. I have simply postponed it from time to time, till this P.M., I saw a report of your cases in P. D. & Co.'s 'Therapeutic Notes.' Inclosed find detailed treatment and temperature charts of two cases. The first of them, Mr. W., was to my mind a typical case. The second Miss J., a trained nurse, came from a family all of whom had typical and severe attacks, at which place she contracted the disease. She came back to Canton and asked me to give her the 'Woodbridge treatment,' as she had nursed my first case, and was much taken with the results obtained in it. By-the-way, she

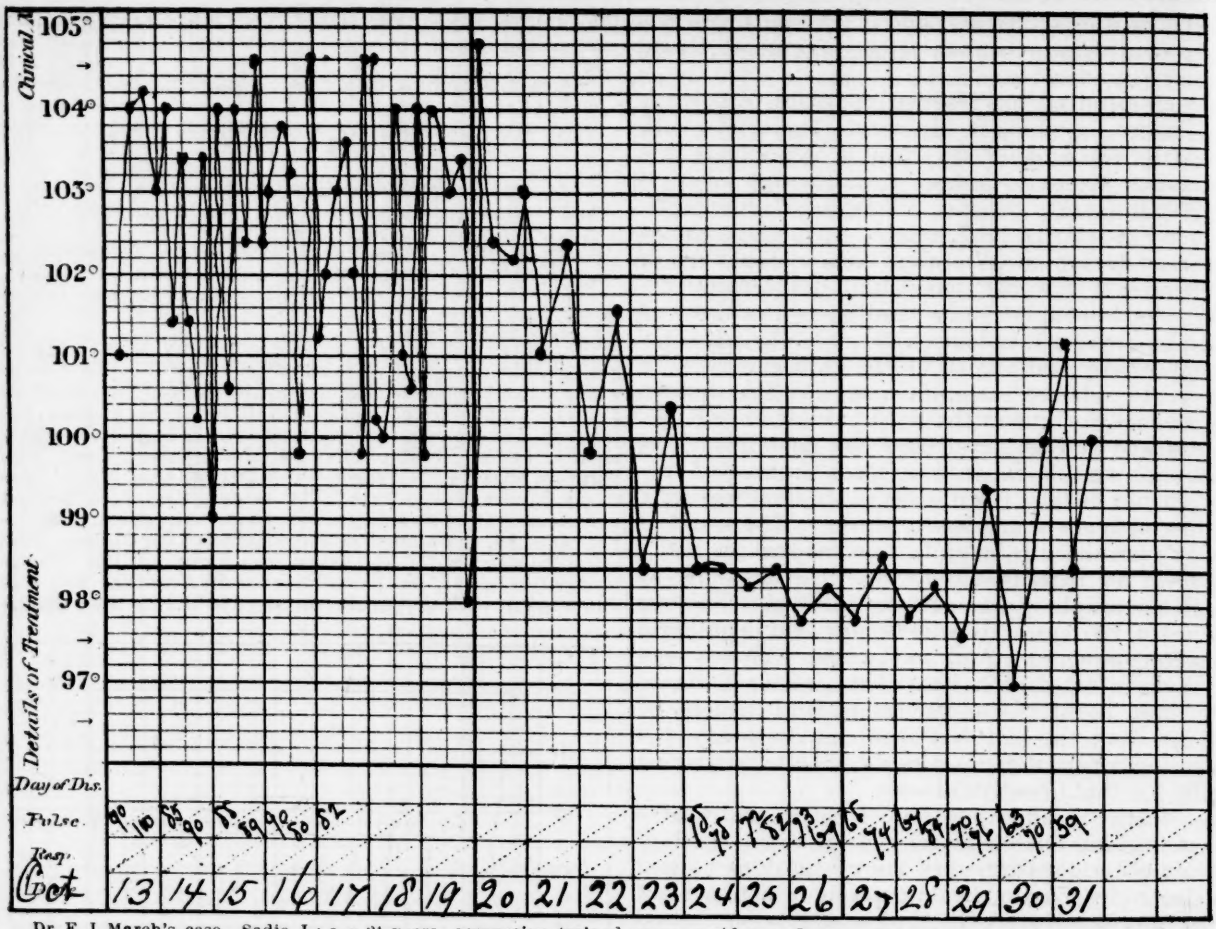


Dr. E. J. March's case. Harry W.; age 25 years; native of United States; occupation, tourist. Diet, milk. Treatment, "Woodbridge's." Result, cured.

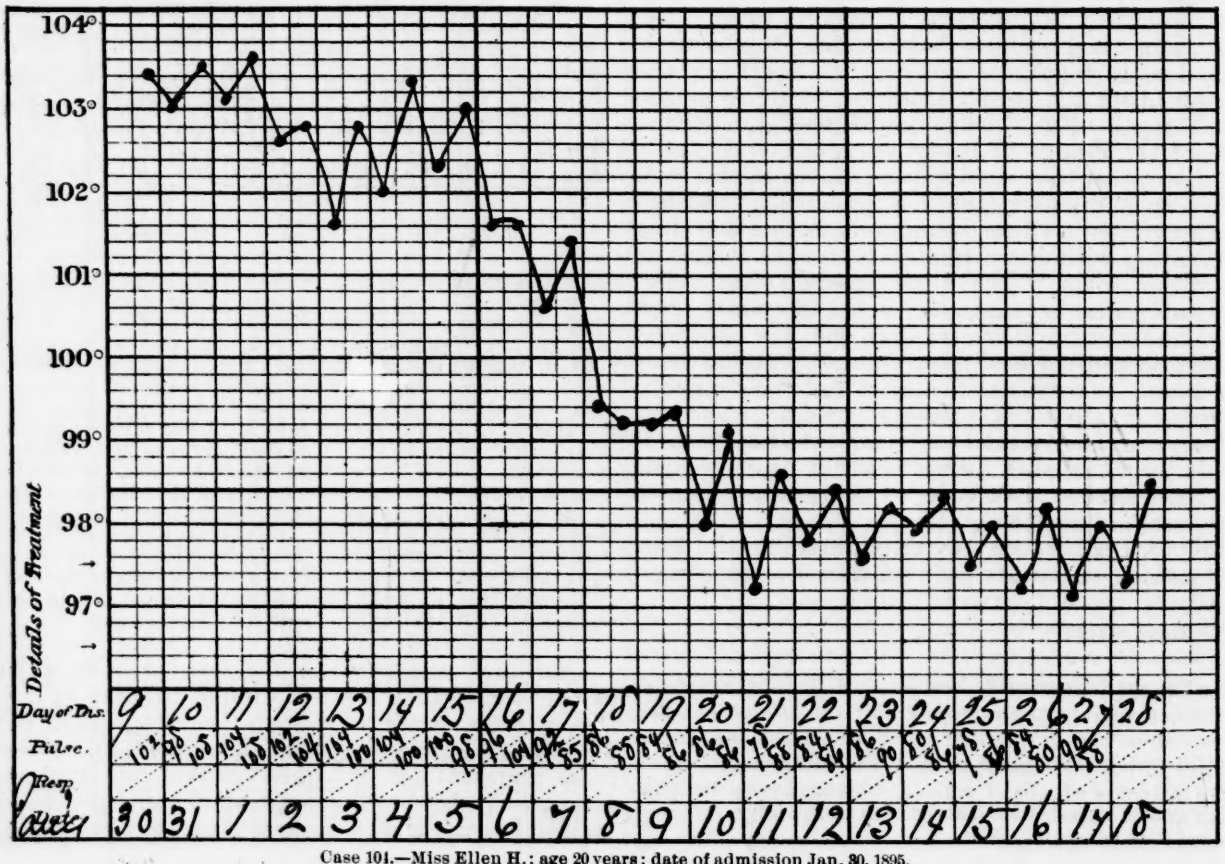
had had large experience in the New York hospitals in nursing typhoid. You will see the results as per charts inclosed. I had two other cases as well marked and typical as these two; but both being in non-professional nurses' hands no notes or charts were kept, but they were convalescent in ten days' time. The one, a woman about 30 years old, had come from the bedside of her brother who died of the disease; and she started in with a severe attack. I am sorry I did not keep such a record as I could, myself, so as to add water to your wheel. I have a friend here who has tried the plan of treatment, somewhat modified, and says that the objection he has to it is that whereas he formerly made from $20 to $40 out of each case of typhoid, this cuts his bills down to less than $10 in each case. Doctor, I feel sure that you are on the right track, and if we should have a run of fever in our hospital wards this fall, I will furnish you with some more charts and results. Thanking you again for your courtesy,

"Yours very truly. E. J. MARCH."

I will now present from my own practice a few cases which ran a course similar to that of Dr. Cunningham's Case No. 3, with the exception of the unfortunate accident of the intussusception. These cases represent not only most careful and positive diagnoses from the clinician's standpoint, generally verified by one or more physicians; but every pre-



Dr. E. J. March's case. Sadie J.; age 24 years; occupation, trained nurse; residence, Canton, Ohio; date of admission, October 13, 1894. Diet, liquid. Result, cured.

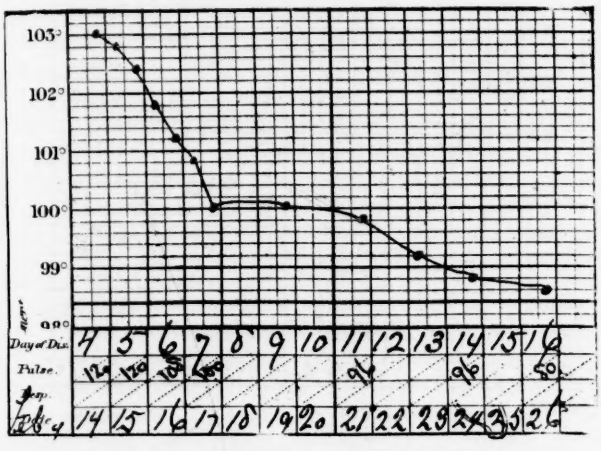


Case 101.—Miss Ellen H.; age 20 years; date of admission Jan. 30, 1895.

caution has been taken to reduce the errors of diagnosis to a minimum by calling to my aid every known means of excluding mistakes. The diazo-reaction described by Ehrlich, and the microscope were appealed to, and in a large number of cases where malarial fever was suspected or had been diagnosticated, careful search was made for Laveran's hematozoa.

Case 106.—Mrs. McC., aged 70 years. Was a feeble woman and one of five typical cases in one house. When I first saw her on February 7 her temperature was 105 degrees, pulse 140. Although she was reported to have been sick only five days she was delirious; her bowels were very tender; there was considerable tympanitic distension; rose-spots were abundant; her tongue was thickly coated, dry and brown. Her temperature, as you will see, had dropped to 99 degrees; her pulse 96 on February 13, after which her temperature was not taken; in a few days she was able to be about, and at the end of a week more she was able to be out of doors.

Case 101.—Lenore S, aged 27 years (daughter of Case 106), residing in the same house with her mother. When I first saw her on January 14, she had a temperature of 103 degrees; pulse 120, and all other symptoms of typhoid fever as characteristic as were those of her mother. A few days later, January 17, her temperature was 100, pulse 100, and the general condition was so much improved that I have no record until January 21, when the temperature had been reduced to 99.8 and the pulse to 96. I saw her the last time on the 26th, and she was out in a week.



Case 101. Lenore S.; 27 years; date of admission Jan. 14, 1895.

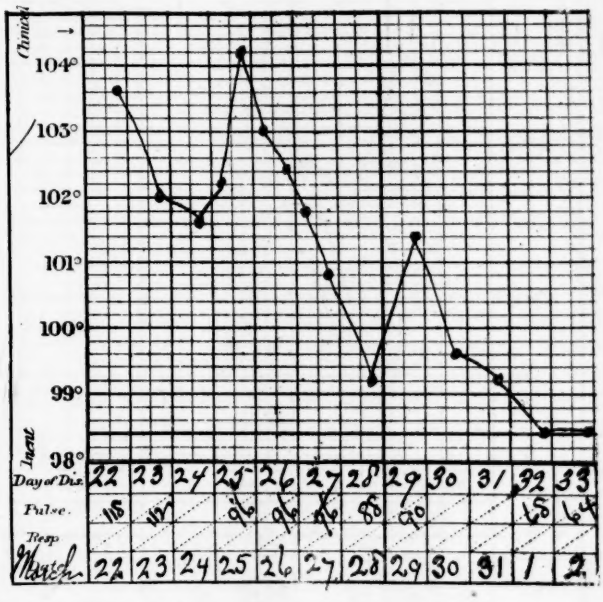
Case 108.—Carolyn S., aged 2 years (daughter of Case 101). She was far from well on February 7, when I was called to see her grandmother, and on February 9 had a temperature of 105.2 degrees. She was discharged cured on the 16th after nine days of treatment.

Cases 107 and 109.—Katie McC., and Jerry McC., aged respectively 5 years and 23 years (granddaughter and son of Case 106). These cases both showed decidedly characteristic symptoms of the disease, but recovered so rapidly that no records were kept.

Case 115.—John O'L., aged 28 years. This young man was taken sick on March 1 and was treated by an irregular practitioner who called two other physicians in consultation after the patient had been ill twenty-two days, during which time (according to the statements of the family) a diagnosis was made of la grippe, malaria, pneumonia and, finally, of pneumonia with typhoid malaria. On March 22, an unfavorable prognosis was given and I was sent for. I found a typical case of typhoid fever, complicated with pneumonia. His temperature was 103 degrees, pulse 118, and dicrotic; condition of nervous system very bad; he was sleepless and dilirious; abdomen was covered with rose-spots; tympanitic distension enormous; bowels very tender; tongue very dry, brown and cracked; he was spitting blood and had three intestinal hemorrhages. On March 25 his temperature was 104.7. However, his condition improved rapidly indeed, he was sleeping naturally, without hypnotics, on the second night, when he was no longer delirious. Within three days his abdomen flattened out; the tympanites disappeared; his appetite returned and he declared that he was gaining strength on a milk diet. On April 1 and the following day, the temperature being normal, he was discharged, cured. Dr. Cunningham, who is much interested in the abortive treatment of typhoid fever, saw the case with me, for the purpose of watching the effect of the treatment. He pronounced it wonderful.

Case 120.—Mary O'L., (sister of Case 115, John O'L.). I was called to see this case on April 24, and was given the following history: she had nursed her brother through a very severe attack of the fever a month before. During the latter part of his illness she had severe headache and general malaise. About two weeks before I saw her, her headache increased; her back and limbs ached; she became dull and apathetic, and as her mother expressed it: "Could hardly drag one foot after the other; she who was the most active and energetic of all the kith and kin." At this time she took the residue of the medicine I had left for another sister, whom I supposed was taking typhoid fever (from which the sister recovered at once) after which she felt quite well for several days. Six days before my first visit, the headache returned; the back and limbs began to ache; and all the characteristic symptoms of typhoid fever presented themselves; her pulse was 120; her temperature was 104.9; her tongue heavily coated; spleen enlarged; bowels very tender and tympanitic.

I left her in the care of Dr. Cunningham, on the ninth day of treatment, when I started to attend the



Case 115. John O'L.; age 28 years; date of admission March 22, 1895. Taken sick March 1, 1895; confined to bed March 4, 1895; spitting blood; tongue dry, margins red, etc.; tympanites; hemorrhages March 23, 24 and 25. Catheterized March 28.

meeting of the AMERICAN MEDICAL ASSOCIATION at Baltimore. During my absence she had a very severe intestinal hemorrhage, but made a good recovery. While Dr. Cunningham was making his daily visits, a sister was taken sick with the fever and proper treatment was instituted early, so the temperature never rose above 103.5 degrees, and on the seventh day, when she was doing very well, her husband came home drunk and brought an irregular practitioner who very promptly decided that she had no fever. It was very fortunate for his reputation that he was called in after Dr. Cunningham had aborted the disease.

Case 116.—E. S., aged 40 years (husband of Case 84). This patient resides about four miles from my office and he drove that distance to see me, about 1 o'clock each day. He consulted me first on April 1, when I found his temperature 100 degrees, and pulse 112. Although feeling at times miserable, he was able to be out each day, as I advised; and on the 15th his temperature was 99.4 degrees, pulse 80, and his recovery was excellent.

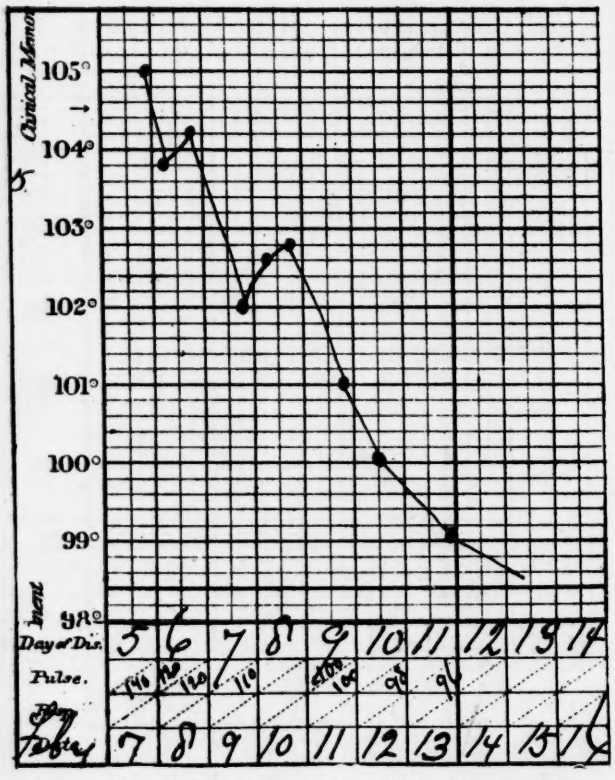
Case 114.—Mrs. Michael J., aged 32 years. (This was the

fourth case of typhoid fever in the same house at once; the other three cases will be reported in the paper which I read in the Section on Pediatrics, at the meeting of the AMERICAN MEDICAL ASSOCIATION at Baltimore.)

(A small son, Case 78, John J., and a little niece, Case 105, Annie W., had had the disease some time before). The symptoms were not well defined and an absolutely positive diagnosis was not possible, but as all the family used water from a well which was supposed to be the source from which several other cases had originated, she was treated as a case of typhoid fever, and assured that if the diagnosis were correct she would not be confined to her bed or be long sick or too sick to nurse her three children, who had the disease, as I had been able to treat her from the very beginning of her indisposition.

Case 110—Frank S. This was a true ambulatory case and although the symptoms seemed to indicate that the patient was growing worse, his temperature being 103 degrees on the sixth day of the treatment, he felt fairly well after the first two or three days.

Case 118.—John S. This case showed premonitory symptoms of typhoid fever for more than a week before the patient consulted me. He drove to my office for a few days,

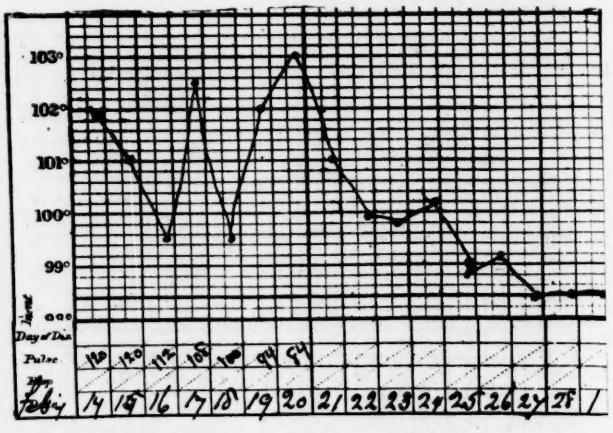


Case 106. Mrs. McC.; age 70 years; date of admission Feb. 7, 1895.

with all the marked characteristics of the disease plainly to be seen, but finally had to go to bed for a week or more. The temperature ranged from 100 to 102.8 degrees for eight days, and he was discharged cured on the twelfth day.

Case 117.—George J., aged 14 years. He came to my office on April 14, and called daily for four or five days, when his afternoon temperature was only 101 degrees; but his other symptoms were sufficiently characteristic to justify a diagnosis of typhoid fever. I soon discovered that he was not doing well and warned him of the danger of neglecting to follow implicitly my instructions; after which he improved for few days, but again grew worse. Sending for his father, I found that he had been left to take his medicine or not, as he chose; and choosing the latter, he had been casting the expensive medicine which I gave him into the cuspidor. I refused to have anything more to do with him, unless he were properly nursed, when his mother gave up her situation and gave him her care; but more than two weeks had now passed, and instead of being well as he should have been, his symptoms were all aggravated; his pulse 136; temperature 103.4; the tympanitic distension of the abdomen was enormous and his bowels were very tender and painful. Leaving him under the care of Dr. Cunningham, I went to Baltimore, and returning on May 13, the Doctor requested me to see him, saying that he was unable to control him. On several occasions he had stolen out of bed and secured bread and other articles of solid food, and once he was said to have eaten a pound of candy. I ordered him to be handcuffed to the bed, where he still remains but is nearly well.

Case 104.—Miss Ellen H., aged 20 years. Residing in a neighboring town, she had been sick ten days under the care of the family physician who, being one of those gentlemen who do not believe in the possibility of aborting typhoid fever, very naturally blundered in his diagnosis and treated her for la grippe, until about four days before I was called. At this time he corrected his diagnosis to typhoid fever, which caused no little excitement, as the other most prominent physician was and is an enthusiastic advocate of the "Woodbridge method." (He says, publicly, that no patient should die of typhoid fever.) Thus the friends of scientific medicine and the friends of "old fogyism" became partisans, and for days the young lady's mother, in her anxiety to save her daughter from death or needless suffering, was swayed from side to side. It would be hard to tell which party would have won had not the condition of the patient become alarming, for although the doctor was keeping the temperature down with acetanilid and sponge-baths, and was sustaining the heart with strychnin and digitalis, her mother could see that all was not right, and she at last telephoned to me to come down and meet the attending physician in consultation. I found both physician and friends exceedingly anxious about a "failing heart," a threatening danger which I assured them would disappear as soon as a little of the poison was neutralized. I discon-



Case 110.—Frank S.; date of admission Feb. 14, 1895.

tinued all heart tonics and stimulants, and all medicine, in fact, except the three prescriptions which I have advised in former papers; and before I made my visit the following afternoon, all the ill-omened heart symptoms had disappeared and the pulse had resumed a healthy tone. In two or three days all of the nervous symptoms, all of the tympanites, all of the abdominal tenderness were gone, never to reappear, and she was very comfortably sick; although, as you will see, the temperature did not touch normal until the eleventh day.

In conclusion, I wish to say that an apology is due the large number of physicians who have reported cases of typhoid fever treated and aborted by the "Woodbridge method" which I have been unable to reproduce here or in previous papers for want of time—an unintentional and unavoidable discourtesy, since the twenty minutes allowed me in the medical societies, or the space in the JOURNAL would have been wholly inadequate to admit of the briefest possible mention of all of the valuable and valued reports in my possession. But every case reported is an object lesson to those who doubt the possibility of aborting typhoid fever, and will aid in hastening the day when death or long or severe illness will be unknown. Hence the medical profession is to be congratulated that it has in its ranks proficient men, able enough to learn the truth and brave enough to teach it.

TOXICITY AS AN ETIOLOGY OF NERVOUS DISEASES.

Read in the Section on Practice of Medicine, at the Forty-sixth Annual Meeting of the American Medical Association, at Baltimore, Md., May 7-10, 1895.

BY ALBERT E. STERNE, A.B., M.D.

PROFESSOR OF NERVOUS DISEASES AND CLINICAL MEDICINE, COLLEGE OF PHYSICIANS AND SURGEONS; CONSULTING NEUROLOGIST TO THE CITY HOSPITAL; PATHOLOGIST TO THE CITY DISPENSARY, ETC.

INDIANAPOLIS, IND.

Of all branches of medicine, that represented by diseases of the nervous system has perhaps been least influenced by the science of bacteriology. In a minor way it has felt the effect of microbic theories, but until now, so far as my knowledge extends, no attempt has been made to explain more rationally upon this basis some of the vexed questions of the etiology of many nervous affections.

I shall undertake to bring forward a newer origin of many diseases of the nervous system. In these days of scientific medicine our profession rightfully demands proofs. I am aware that I have little incontestable proof in support of this theory to offer at the present time. Nevertheless, I think considerable evidence for the toxic origin of many affections of which the etiology has long been a source of doubt will be shown.

In speaking of the toxic origin of nervous diseases it was intimated that the idea was new. This is not entirely true. It is well known to you that of late years no inconsiderable rôle has been attributed to toxicity as an etiologic factor in a number of maladies. But in medical literature I have found little recorded relating to affections of the nervous system attributed to the effect of bacteriologic or analogous influences. For a long time it has become impressed upon me in my special study of nervous diseases, that the degenerations, respectively inflammations, of both the central and peripheral nervous organs are, in a great measure, due to the effect which certain discovered and undiscovered toxins exert upon them. In some instances this result is attained only after an extended period of time; in others the effect upon the nervous tissue is quite acute as, for example, in poliomyelitis anterior, the so-called "infantile paralysis." In some cases the toxin is the product of microörganisms, as in diphtheritic and typhoid affections; in others the poison is of an inorganic nature, as is the paralysis due to lead, arsenic, mercury and alcohol.

There are several reasons which prompt me to seek the origin of many abnormal conditions of the nervous system in a chronic or acute exposure to the influence of toxicity. It will be impossible to enter deeply into the subject, for the realm of nervous diseases is vast, and the number of agents which may be powerful to exert their action possibly still greater. I must necessarily simply allude to many things to which further study and experiments may attach considerable importance.

Now it has been the just aim of pathologists and neurologists for many years to attribute the lesions of well-known diseases of the nervous system to defects of a congenital nature. These defects vary in degree, and in some instances require the assistance of an exterior agent to unfold to such an extent as to impress us with their importance as factors producing disease. With this tendency and aim I am most heartily in accord, as is shown in a recent paper by myself on "Syringomyelia."[1] It may be well, however, to caution against a too great willingness to search for congenital deficiencies or "retarded development," to the neglect of other causes, as in almost every case where such an etiology is apparent, prognosis is grave, except, perhaps, where surgical methods can be adopted.

We are all well acquainted with the patient's story of exposure to cold, to wet, to heat, etc., and doubtless such exposure, if it be long continued, can produce serious effects upon the entire nervous system, in fact upon the entire body. The popular notion concerning exposure is, perhaps, not so ridiculous as many of our fellow-physicians would have us believe. However, these histories of exposure of various kinds are to the inquiring mind unsatisfactory, to say the least. Guided by the light which the researches of the latest years have shed upon every field of scientific medicine, we should attempt to arrive nearer to the true causes in the production of all diseases. Therefore I would bring forward the following facts:

1. We find as sequelæ of affections of known microbic origin, inflammations and, secondarily, degenerations in both the central and peripheral nervous elements.

2. After and during the course of such illnesses as may be justly supposed to be due to bacterial invasion we find characteristic symptoms from the side of the nervous system denoting organic disease of the tissue of this system.

3. By analogy we may infer that both of the former classes are due to the effect of the microbes themselves or to the production of poisons by them.

4. This analogy is given by the two following subdivisions: (*a*), the inflammations and degenerations found in the nervous tissues as the result of toxins of non-microbic character; and (*b*), those notably due to the presence in the nerve elements of microörganisms or their products.

Under these classes we find affections of acute, sub-acute and chronic character. Again, among these we find affections of peripheral and central nature, both organic and functional. Into these conditions, which appeal to almost all of us as results of poisoning of shorter or longer duration, I shall enter but little, even though their importance for the worth of the toxic theory is of great value. I refer particularly to the nervous sequelæ or complications which follow the acute exanthemata and other febrile diseases. Some of these are undoubtedly, and others manifestly, due to the presence of microbes in the system. Of the former I mention diphtheria, typhoid and influenza as paradigms; of the latter, scarlet fever and measles. Paralysis, usually of peripheral origin, is comparatively frequent after the first named, seldom after the latter. It is still an open question as to the specific agent producing these paralyses, whether it be the bacteria themselves or their toxins. It seems to me that the cause must be sought in the action of the latter alone. As has been intimated, these paralyses are due to inflammations and often consequent degeneration of the peripheral nerves. We know that peripheral neuritis is frequently of toxic nature; as is seen in cases of lead and alcohol poisoning.

Speaking more particularly of affections of the nervous system we have among the acute forms due to

[1] Indiana Medical Journal, December, 1893.

toxic influences of *bacterial* nature, those presenting the clinical and pathologic conditions due to destruction in the central nervous organs. First, the poliomyelitis anterior or "infantile paralysis" with degeneration and atrophy of the ganglion cells in the anterior cornua of the spinal cord; also meningitis cerebro-spinalis epidemica and inflammations of the meninges, due to tubercular and other bacillary forms. Those rarer diseases, such as encephalitis and myelitis acuta appear from their clinical and pathological character to be toxic. Of the acute peripheral affections produced by the microbic toxins, I cite "beri-beri" and, in repetition, the diphtheritic pareses and paralyses and the ascending paralysis of Laudry.

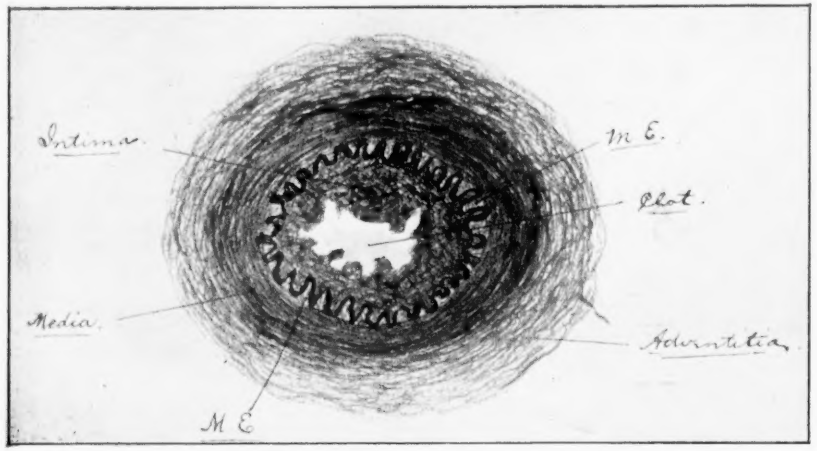
It will be vastly more difficult to establish a toxic origin for those conditions, both organic and functional of the nervous system, which are commonly denominated as nervous diseases. The suggestion that the functional disorders which show themselves as epilepsy, hysteria and the mental perversions both of depression and exaltation as the direct manifestations of toxic action lies very near. Assuredly many parallels could be drawn to express the similarity in the effects of known poisons upon the cerebral and spinal functions, and the above mentioned affections. In this connection it will be necessary simply to allude to the action of strychnia, and of curare among drugs, and to recall the symptoms often supervening from the results of intestinal, biliary and renal intoxications. Disturbance in the physiologic action of many glands produces chronic nervous ailments. So for instance, is this true of the thyroid and presumably of the thymus where this persists after puberty. It is an achievement of modern science which has given to us a knowledge of myxedema, and of exophthalmic goitre, both of which seem due to the chronic action of some still obscure toxin.

That syphilis is due to bacterial infection there can be no reasonable doubt. That of extra-congenital conditions of nerve lesions we should seek the etiology preëminently in the action of the syphilitic virus may be considered almost equally true. The three agents which play a main rôle in the production, better said, induction of nervous diseases, both organic and functional, may be said to be syphilis, tuberculosis in some form, and alcohol. All three act, it seems to me, in a similar manner, by gradual intoxication. The channel through which this action is spread lies in the blood and lymph currents. The action of the poison is primarily exercised upon the vascular and lymphatic systems, the result of which is shown in subsequently produced lesions in the nervous elements. The above statements are liable to misconstruction. To become more explicit, I believe, not that many diseased states of the nervous system are either syphilitic or tubercular or alcoholic —though for each of these there exists a distinct specific disease of both central and peripheral nature—but that the ptomaines, respectively toxin, produced by constitutional syphilis, tuberculosis or alcoholism are factors which contribute more largely even, than is generally supposed to the development of various diseases of the nervous system. That an anti-syphilitic, an anti-tubercular or an anti-alcohol cure may have little or no effect upon the diseased nervous state against which the one or the other may be directed, seems to me to carry little weight against the etiologic importance of these specific troubles in the production of nervous affections, as some authorities contend. We have in these cases to do with the results of specific infections, not with the infections themselves.

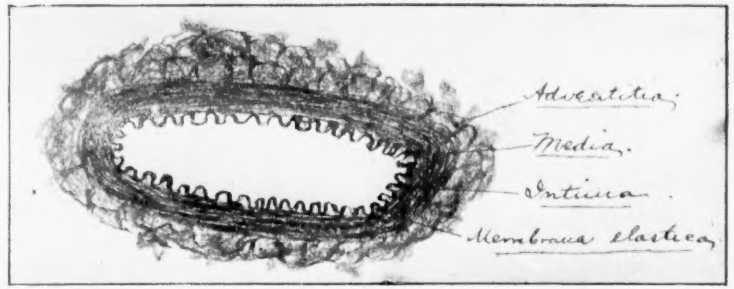
The most common and illustrious examples of the result of chronic bacterial toxicity are tabes dorsalis and lepra anesthetica. These two affections have many similarities, though in fact they are entirely different processes. Whereas in lepra we find the specific bacilli in the nervous tissues, no such conclusive proof of the purely microbic origin of tabes has ever been discovered, and probably never will be, inasmuch as the systemic degeneration which forms the pathologic basis of locomotor ataxia may be best looked upon as the result of some primary process, *i. e.*, chronic toxicity. It seems to me to be an error to regard the only nerve lesions present in tabes (degeneration of the posterior tracts of the spinal cord) to lie in the central nervous organs. This lesion is doubtless the most important, but there are others, notably multiple degenerative peripheral neuritis, equally constant and, moreover, responsible for many of the characteristic symptoms of the disease.

Tabes should be regarded as a constitutional process, one which places distinct and separate lesions within the nervous system, and perhaps in other tissues. It is in the vast majority of cases a post-syphilitic affection, the direct consequence not of syphilis but of the ptomaines or toxins produced by the agents of the specific disorder. That few cases of well-developed locomotor ataxia are in any way benefited by mercury and iodid of potash need not appear strange. The action of these drugs directs itself against the microörganisms and their ptomaines, not against their secondary effects. That the syphilitic agent acts most powerfully upon the vascular system of the nervous organs can be readily demonstrated. The characteristic changes in the blood vessels of the central nervous organs are present in almost every microscopic section. To these changes of the vascular system the degenerations throughout the whole body of the tabetic individual may be, to a great extent, directly traced. Just how the process is to be explained, whether through inanition due to defective oxidation on account of the condition of the vessel walls, or whether the poison exerts direct influence upon the tissues supplied by these vessels, or whether the changes spread from the vessel walls, remains a matter of doubt. It seems certain, however, that the degenerations within the nervous system are changes secondary to those of the vascular system. They are interstitial, not parenchymatous. Moreover, I regard both as consequences of toxicity.

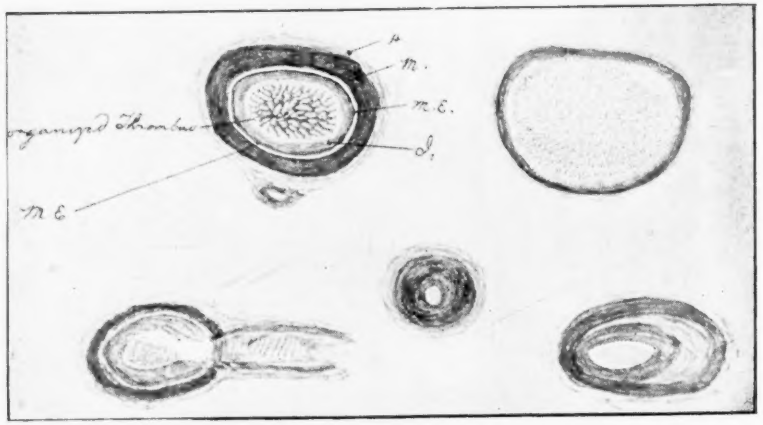
Whether or not it will prove possible to establish analogous relations for the many other essentially nervous diseases, amyotrophic lateral and multiple sclerosis, for the non-systemic degenerations, etc., I am not now prepared to say. However, we have affections of essentially nervous character which appeal to us even more, namely, tetanus and chorea. That tetanus is the result of the action of tetanim or tetanotoxin is beyond question. Experiment has proved that this product of the tetanus-bacilli causes the disease. Of the alkaloids, strychnia acts much in the same way. Whether the affection called tetany is also of a toxic nature it is yet impossible to state. Its endemic nature might lead us to infer that it is. That chorea associates itself very frequently with an



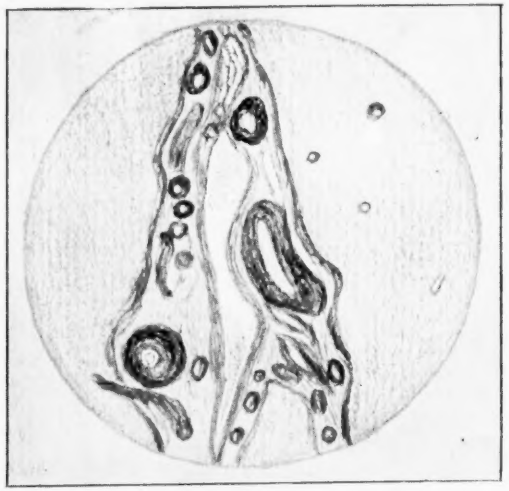
Endarteritis luetica, lues cerebro-spinalis. Very small brain artery. M E, Membrana Elastica.



Combined Sclerosis. Direct cerebellar tract, antero-lateral ascending tract, and Goll's columns. Vascular changes. Spinal artery. Adventitia, much broken up and thickened. Media, much thickened. Intima, almost intact.



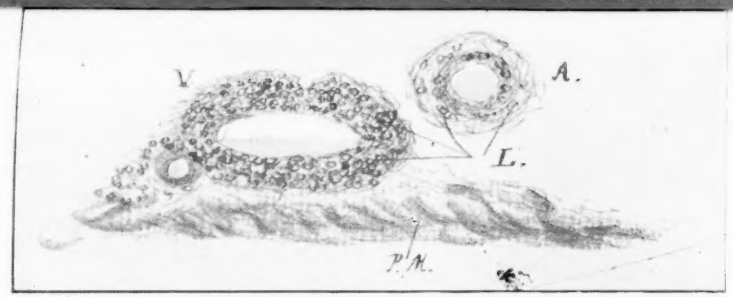
Endarteritis specifica, lues cerebro-spinalis. A, Adventitia. M, Media. I, Intima. M E, Membrana Elastica.



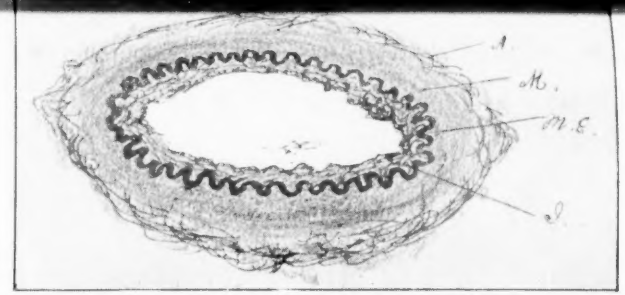
Endarteritis specifica, lues cerebro-spinalis.

Endarteritis specifica, lues cerebro-spinalis. A, Adventitia. M, Media. I, Intima. M E, Membrana Elastica.

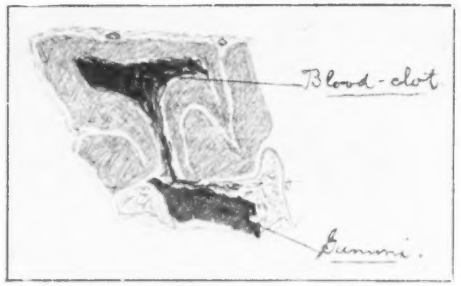
Endarteritis specifica, lues cerebro-spinalis.



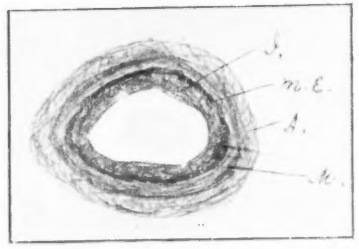
Combined Sclerosis (25). Pyramidal tracts and Goll's columns. Vascular changes. Walls of both vein and artery much thickened and destroyed. Leucocytes (L) are to be seen distributed throughout the vessel walls, notably in that of the vein (V). P M, thickened pia mater.



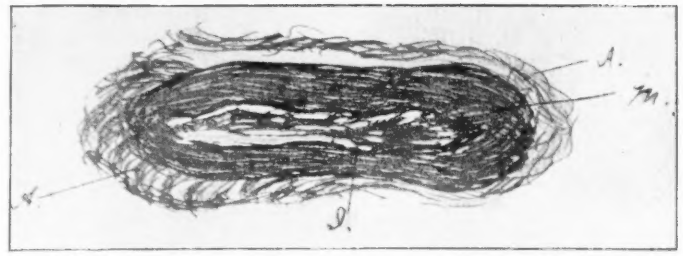
Syringomyelia. Vascular changes. Spinal artery. Adventitia (A) much thickened and fragmented. Media (M) very much thickened. Intima (I) fragmented and loosened; much thickened. M E, Elastic Membrane.



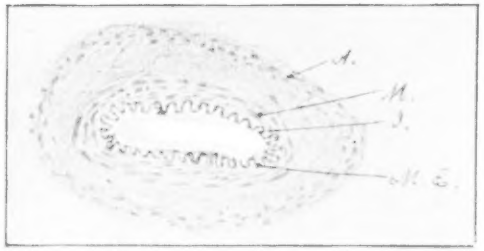
Hemorrhage and subcortical gummi, lues cerebralis.



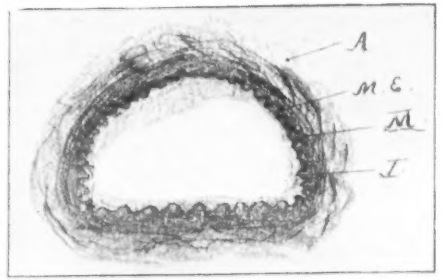
Tabes Dorsalis. Vascular changes. Minute spinal artery. Adventitia (A) thickened and in fragmentary destruction. Media (M) thickened. Intima (I) much thickened. M E, Membrana Elastica. Thrombus in artery.



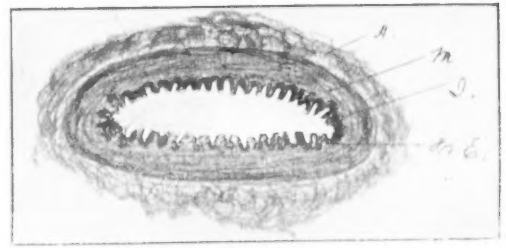
Poliomyelitis Anterior. Vascular changes. Spinal artery. Adventitia (A) very much fragmented and loosened from media. Media (M) much thickened. Intima (I) torn from the media.



Gliosis Spinalis. Vascular changes. Small spinal artery. Stained to show proliferation of nuclei. Adventitia (A) much thickened, many nuclei near the periphery. Media (M) thickened, countless nuclei. Intima (I) somewhat loosened; no proliferation. M E, Membrana Elastica.



Amyotrophic Lateral-sclerosis. Vascular changes. Small artery. Adventitia (A) fragmented. Media (M) thickened. Intima (I) loosened and thickened. M E, Elastic Membrane.



Syringomyelia. Vascular changes. Small spinal artery. Adventitia (A) thickened and broken up. Media (M) much thickened. Intima (I) nearly intact. M E, Elastic Membrane.

undoubted infectious disease, namely acute inflammatory arthritis, seems to me to prove at least the possibility of its toxic origin in the majority of cases.

Many other examples could be brought forward. Our daily experiences teach us many. We meet with the nervous sequelæ of the infectious fevers. We look for them from the action of poisons, either taken from without or generated within the body, arising from disordered functions in various organs, absorbed from the intestines or taken into the circulation from liver and kidneys. We seek the candidates for nervous and mental diseases preëminently from among those long exposed to chemic intoxication both of organic and inorganic character; in other words, from those subjected to toxic influences and the abuse of drugs and alcohol. Dementia paralytica may aptly be mentioned here.

Acting upon this line of thought and experience, I have carefully examined many specimens of the nervous system taken from different subjects suffering from the same affections, and several of these specimens I wish to submit to your inspection. In this investigation especial attention was devoted to the condition of the blood vessels, notably in cases of tabes, syphilis, dementia paralytica, amyotrophic lateral sclerosis, multiple sclerosis, primary lateral sclerosis, poliomyelitis and myelitis acuta, syringomyelia, gliosis spinalis and various combined systemic scleroses, among the organic diseases. Of the so-called "functional" affections, epilepsy and chorea were studied. Fatal cases of diphtheria and scarlatina in which no paralysis had supervened, and of the various cachexias, notably of tubercular subjects, of profound anemia due to intestinal intoxication, and of alcoholists were also examined, in order to determine how far the vascular changes might be due to toxic action or simple inanition, in cases which had shown no essentially nervous symptoms. In all of these affections the condition of the vessel walls differed markedly from the normal. It can not be said that there was any constant degree of pathologic alteration in any one case or in a given disease. The character of these vascular changes you can easily detect in those specimens beneath the microscope, and from the drawings near them. Unfortunately the detailed structure is not given in the latter. Taken altogether it will be seen that the walls of the vessels present very decided differences from the normal. The pathologic process appears more marked in cases of syphilis and in post-syphilitic tabes and dementia paralytica than in the other diseases. It seems more intense and widespread. However, it will be noticed that in those affections which have heretofore been less studied as regards their vascular condition than the above, advanced disease of the vessel walls is shown in many cases.

All the layers of the arteries are affected by the process, sometimes the one more than the others. In the main the process is one of cell-proliferation effecting increase in the volume of the vessel wall at the expense of the vessel lumen. Even where considerable fragmentation of the vessel walls has occurred, and where the intima or adventitia are loosened from the media this increase persists. Very often the lumen of the vessels is entirely obliterated just as in endarteritis syphilitica. In several specimens enormous quantities of leucocytes were present in the vascular tissues. In several instances slight hyalin degeneration was present, but in no case observed by me was this peculiar change constant or widely diffused. Thrombosis seems more common in the more chronic, fragmentation more usual in the acute diseases, such as poliomyelitis and myelitis. In the cases of diphtheria and scarlatina, which were all rapidly fatal, evidences of acute inflammation and extravasation predominated over other changes. Fragmentation, *i. e.*, varying degrees of tearing and loosening of the intima, media, and adventitia, also obtained.

In the cases of epilepsy and chorea which I examined, the condition was closely allied to the above, minus the signs of acute inflammation. The innumerable minute hemorrhages present in these cases certainly indicated that some agent, probably toxic, had exercised considerable influence upon the vessel walls. It seems to me that this same toxic agent acts upon the nerve cells, perhaps only functionally or chemically like certain drugs, and causes the clinical symptoms of these affections.

It is interesting to note that the state of the vascular system in the marantic conditions investigated, namely in the cachexias of tubercular, profoundly anemic and alcoholic subjects showed no great divergence from that described above. The quality of the pathologic changes was very similar, only the intensity of the process was less marked. It seems, therefore, that in all of the affections in which some degree of toxicity was manifest, that the blood vessels are more or less intensely altered. These changes I consider the direct result of some toxic action, possibly chemical in its nature. In how far the period of exposure acts in producing varying grades of lesions it would be difficult to say. That the duration of time in which the toxic agent has exerted its influence may be a factor of great consequential importance can not easily be denied, when we reflect, that, in a notoriously long-lived intoxication *i. e.*, syphilis, the vascular lesions are most intense and widespread. How it is that different parts of the nervous system possess decided affinity for certain toxic agents it would be difficult to determine. That such an affinity exists, I belive that no one can gainsay. How else can we explain rationally the predilection with which lead attacks the radical group, or the diphtheritic toxins certain nerve territories? Functional overwork undoubtedly plays no inconsiderable rôle in this selective and elective play, but some peculiar chemic affinity seems clearly at work in many afflictions following given types. We are still far from a clear conception of nerve chemistry, the proper knowledge of which would render these problems more easily solved, and eventually teach us the way to reconcile our bedside and consultation experiences with pathologic processes and theories of diseases.

75 E. Michigan Street.

DISCUSSION.

DR. PRESTON, of Baltimore—I think the subject presented to us is one of extreme importance, and one which certainly is attracting the attention of neurologists very greatly. It is almost too vast to discuss, but there is a part of it that has attracted my attention for several years. I was particularly pleased to see the importance which Dr. Sterne attaches to vascular changes. There is one affection, namely, posterior sclerosis (tabes) which I think shows that in a very interesting and remarkable manner. If we consider that we very rarely ever have a cervi-

cal tabes, that is I think of itself a fact worth noticing. A few cases have been reported of purely cervical tabes but this is unusual. If we recall the blood supply of the spinal cord, we see that the lower part of the cord is supplied mainly by the branch of the intercostal artery, the upper portion of the cord getting very little from the artery. Here is a small branch coming off at right angles from the trunk, and the sclerotic material is heaped up just at the mouth of this intercostal artery. That being the fact, we see at once that the nutrition of the lower portion of the cord is certainly much diminished by this change in the intercostal artery, so that instead of getting its proper blood supply, the lower part of the cord does not receive proper nutrition. I think the branch of the intercostal supplies mainly the lower part of the cord, and if that can be demonstrated we could say, I think, that a large part of the cause of tabes was due to the fact that the proper nutrition was cut off on account of the arterial sclerosis of the intercostal arteries, just the arteries we would expect to find affected first. We have a great many toxic causes, alcoholism standing prominently forward, gout, rheumatism, lead poison, etc., and syphilis, all contributing to the production of arterial sclerosis, and these causes certainly act in the production of locomotor ataxia. The syphilitic process, while producing an arterial sclerosis unlike the other sclerosis of gout and lead poisoning, does produce a distinct change. Some years ago I began using nitro-glycerin in the treatment of early tabes, and I am satisfied that I have obtained some good results. I have been able to relieve the pain in early cases of tabes, I am sure, by the continuous use of nitro-glycerin, acting upon the idea that I might produce in that way a slight increase of nutrition to the spinal cord. I think that the fact that certain local applications have proved of advantage is perhaps due to that.

DR. PATRICK, of Chicago—I should like to vote a very decided aye, in regard to the first part of Dr. Sterne's paper, and a very decided no, in reference to another part of the paper. As to the influence of the various toxic agents upon the nerves I do not think there can be much difference of opinion. The influence of the various agents has been shown so conclusively that we are scarcely on debatable ground. Even toxico-pathic heredity has been very conclusively proved. This has long been known and generally established in regard to syphilis, morphin, etc. Parents subjected to lead, mercury, bisulphid of carbon, also beget children with neuropathic tendency. When the Doctor goes a step further, and would attribute almost all diseases of the nervous system to an affection which begins in the blood vessel, I think he will have extreme difficulty in maintaining his position. To group all these affections which he has enumerated, including locomotor ataxia, acute anterior poliomyelitis, etc., under one head, is decidedly going further than our present knowledge of pathology would justify. Our present knowledge would vote a decided no, to these propositions. These affections begin in the nervous structures themselves, and the change is in the nervous conditions primarily, and not in the blood vessels. When these diseases have gone on into an advanced state, we find a change in the blood vessel, but that is secondary. Tabes has been conclusively shown not to be a disease of the blood vessels. These cases begin almost always in the nerve cell, or in the nerve fiber, consequent upon atrophy, destruction, or disintegration of the nervous element. The connective tissue may take on changes, but these are secondary; it is not primarily a disease of the blood vessels at all. Even the changes which have been recorded chiefly by Nunez which were changes in the cord occurring in pernicious anemia, were primarily of the nervous tissue.

DR. STERNE—Dr. Patrick misunderstood me when he thought I classed all these cases together. I did not say that I class all these cases together, except in physiologic importance. It is very difficult, if not impossible, to say where you have these changes, whether they are secondary or primary, in cases of such long standing as tabes dorsalis. I do not believe that the ganglionic cells or the nervous fibers of the axis cylinder are primarily affected.

PERITONEAL SUPPORTS—(LIGAMENTUM PERITONEI).

BY BYRON ROBINSON, M.D.
PROFESSOR OF GYNECOLOGY POST-GRADUATE SCHOOL. CHICAGO.

(*Continued from page 67.*)

THE GREAT OMENTUM.

Synonyms: mesogastrium posticum, posterior mesogaster epiploön, gastro-colic omentum, omentum majus, ligamentum gastro-colicum, apron or caul.

The great omentum is a double fold of peritoneum which stretches from the mid-dorsal line to the greater curvature of the stomach and concave surface of the duodenum. The mesogaster posterior and the meso-duodenum are one and the same mesentery. It is twisted into the shape of the letter S, with one loop of the S larger than the other on its

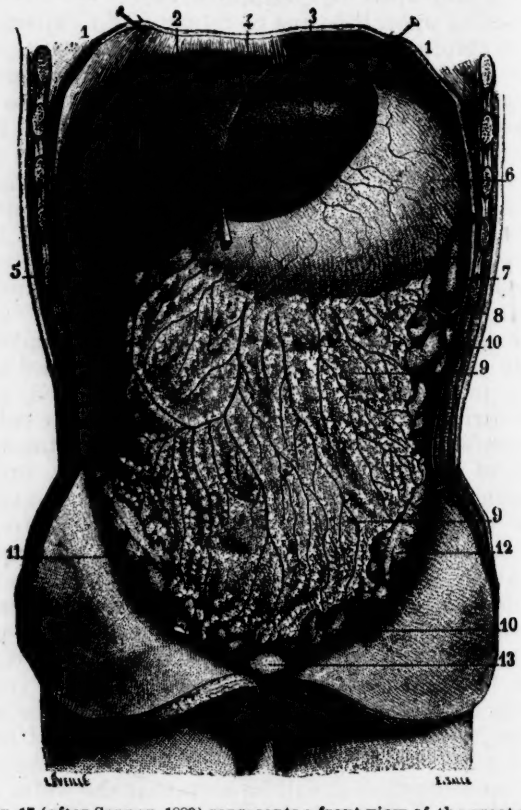


Fig. 17 (after Sappey, 1889) represents a front view of the great omentum. It is unusually well spread out and quite symmetrically located. It shows well the relations of the great omentum to the surrounding organs, especially the three great peritonitic regions, viz.: gall bladder, appendicular and pelvic. It does not cover the cecum (No. 11), nor does it reach the pelvis, and the top of the gall bladder (No. 5). 13, summit of bladder; 12, *s*, iliac; 10, points to loops of small intestines.

stomach aspect. The ligamentum hepato-duodenale has dragged the pylorus toward the right from the atrophy of the liver. The mesogaster (posterior) or the great omentum is very long and wide, and easy to inspect. But the mesoduodenum is so changed by the rotation of the great intestinal loop that it is not readily inspected. However, if one dissects up the transverse colon it will be seen that the whole mesoduodenum is still in existence except the serous covering which faces both sides of the mesoduodenum in early fetal life. In other words, the membrana mesenterii propria is still existing, minus its serous shining epithelium. One can dissect out the mesoduodenum and note the arches and arcades of blood vessels just as they are in the mesentery of the small intestines. The head of the pancreas lies im-

bedded in the membrana mesenterii propria as I have often proved by dissection. The great omentum in early fetal life is a simple double-bladed membrane which suspends the stomach and duodenum from the mid-dorsal wall. As time and growth progresses the mesogaster elongates. It stretches and elongates toward the left before the moving stomach. The mesogaster is at first a plain surface, later a shallow wide-mouthed bag and finally in the shape of a jug with its mouth at Winslow's foramen. The great stomach curvature which was originally at its posterior border has moved to the left, and by so doing the mesogaster has become extraordinarily long. The movements of the stomach (chiefly due to the liver) viz., axial rotation and twisting, with descension are supposed to be the main factors in regard to the elongating of the great omentum or mesogaster (posterior). However, these forces are inadequate to explain the excessive length which the great omentum attains. In man or the higher mammals the rôle played by the transverse colon aids in explaining some factors as regards the length of the mesogaster. Yet some obscurity still exists, for in the dog where the transverse colon is not connected with the great omentum there exists even a longer great omentum than in man. If one could place the fist against the right face of the mesogaster in a

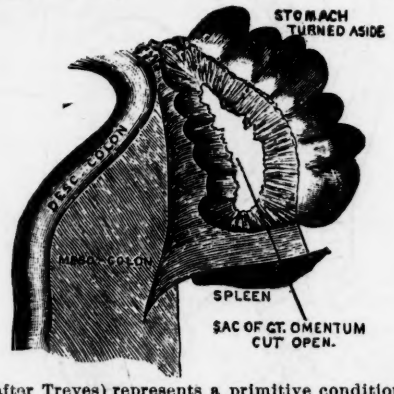


Fig. 18 (after Treves) represents a primitive condition of the great omentum found in the kangaroo. The omental sac is cut open to show that it has not formed a long pendulous omental bag, as in many mammals. In this animal the posterior mesogaster appears as if it were simply pushed slightly toward the left, forming a shallow, wide open depression with no Winslow's foramen. The neck, or rather circumference of the depression in the mesogaster never contracted to form a foramen.

fetus and continue pushing toward the left until the mesogaster was stretched into an irregular sac, the bag could be observed at the left of the stomach while its neck would be found at the right and behind (Winslow's foramen). The walls of the bag consist of a double-bladed membrane, the stomach mesentery which continues to elongate until its walls pass in front of the small intestines and normally to the pelvis. The layers of the great omentum start from the great stomach curvature where they arise from the convergence of the anterior and posterior serous blades of the stomach and pass downward in contact with the anterior parietal peritoneal layer. After descending normally into the pelvis, these two layers turn or reflect on themselves and pass upward in front and in contact with the small intestines until they reach the transverse colon, whence by divergence they embrace the colon and again come together to form the transverse mesocolon. The transverse mesocolon passes to the back wall of the abdomen where the upper blade ascends along the dorsal abdominal wall, while the under blade descends on the dorsal abdominal wall. The chief variation in the mammalian peritoneum exists in the great omentum especially in reference to its relation with the transverse colon.

By reference to Fig. 18, a beautiful illustration of the great omentum of a kangaroo may be observed. In this cut the slightly pendulous bag of omentum is cut open to better show its construction. The part of the mesogaster which bulges is about midway between the pylorus and lower end of the esophagus. This is the case as far as I can determine in the

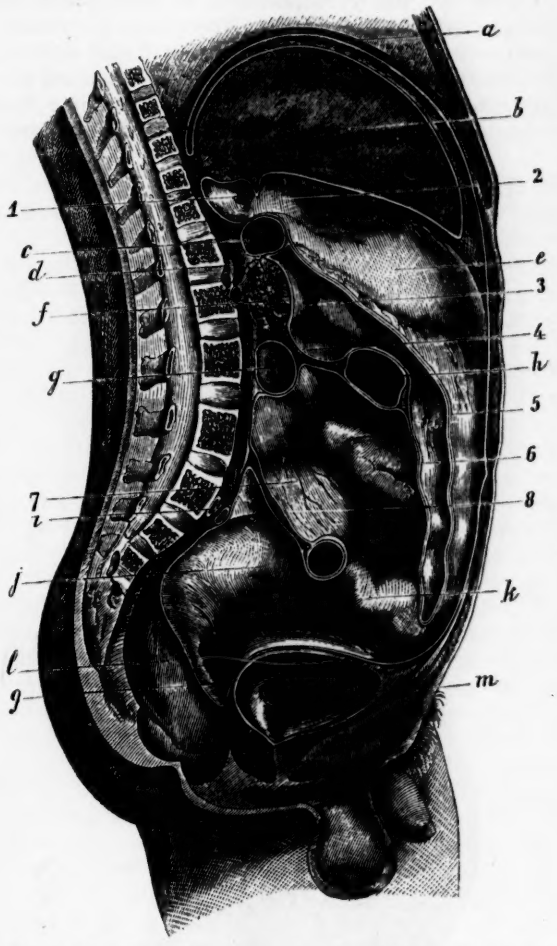


Fig. 19 (after Forabeuf) represents a beautiful profile view of the great omental bag. It is worthy of careful inspection. It is drawn in vertical section in the middle line; *a*, diaphragm; *b*, liver; *c*, duodenum; *d*, renal vessels; *e*, anterior face of the stomach; *f*, pancreas covered by the ascending or superior blade of the transverse mesocolon; *g*, inferior transverse colon; *h*, colon transversum; *i*, iliac vessels; *j*, S-iliac; *k*, small intestine; *l*, rectum; *m*, bladder; 1, Winslow's foramen (orificium epiploon, foramen bursa omentalis pinum or hiatus Winslowii); 2, gastro-hepatic omentum, lesser omentum, or ligamentum gastro-hepaticum; 3, the lower blade of the lesser omental cavity, or the upper blade of the mesocolon transversum ascending over pancreas; 4, mesocolon transversum; 5, anterior or descending blades of the great omentum; 6, posterior or ascending blades of the great omentum (the part below the colon Huschke called the omentum colicum, and the part between the stomach and colon the gastro-colic omentum); 7 and 8 point to the upper blade of the mesenterium, which consists of three layers, (*a*) an upper and (*b*) a lower, shining, serous epithelial layer, and (*c*) a middle layer (membrana propria mesenterii); this middle layer is the real neuro-vascular visceral pedicle, composed of blood vessels, lymphatics, nerves, elastic and fibro-connective tissue; 9, Douglass' pouch (excavatio-recto vesicles). It may be observed that the shining serous epithelial layers may be displaced or entirely dragged off of a viscus, but the middle layer, the membrana mesenterii propria is never displaced or dragged off of its original viscus, e.g., the pancreas in the human fetus lies in the gastro-colic omentum very distinctly, as it plainly does in the dog. In the adult human the shining, serous, epithelial peritoneal layer does not surround the pancreas, but simply lies against its anterior face. The epithelial layer has been displaced or readjusted. But the pancreas is enveloped by the middle mesenteric layer, i.e., by the membrana mesenterii propria, which holds in order as it always did the vessels, nerves and fibrous tissue. Again the duodenum is similarly deprived of its peritoneal epithelium on its posterior surface by displacement or readjustment; but the duodenum is definitely enveloped as it originally was by its real neuro-vascular visceral pedicle, the membrana mesenterii propria or middle mesenteric layer.

fetus of man and animals. If the largest bulge in the mesogaster does exist halfway between the cardiac end of the stomach and pylorus, it accords with what seems to me the method of formation of the omentum and that is through stomach movements. It appears to me that the existence of the serous membrane or peritoneum is due to free moving viscera, plus fixed and independent surrounding walls, *i. e.*, serous cavities depend on the coalescence of interstitial (lymph) spaces from fluid pressure and independent action of surrounding or adjacent walls. In the case of the great omentum its appearance is chiefly due to the liver. The liver enlarges and pushes the stomach toward the left and downward. The stomach coincidently dilates and contracts. The liver enlarges so enormously in fetal life that it elongates the esophagus below the diaphragm and

Fig. 20 (after Luschka, 1863) represents a sagital section of the peritoneum. It is an excellent cut. It may be noted that the peritoneum does not descend sufficiently on the anterior surface of the uterus. 1, navel, with adipose collection; 2, diaphragm; 3, liver; 4, stomach; 5, colon transversum; 6, intestine; 7, rectum; 8, uterus; 9, bladder; 10, pancreas; 11, pars horizontalis inferior duodeni; 12, aorta abdominalis; 13, art. iliaca communis dextra; 14, vena iliaca communis sinistra; 15, bursa omentalis minoris; 16, omentum minus; 17, omentum majus; 18, mesocolon transversum; 19, mesenterium; 20, Douglass' pouch (excavatio recto-uterina); 21, utero-vesicle pouch (excavatio vesico-uterina). Notice that Luschka shares the views of coalescence and not displacement at No. 18, which shows four layers instead of two layers for the mesocolon transversum.

forces the stomach to the left and downward to a surprising degree. The force that twists the stomach so much (as in the pig and dog) appears to reside in the irregular and unequal growth of the liver lobes. At one time in fetal life the left lobe of the liver enlarges so rapidly as to force the stomach extensively to the left and downward. Spigel's lobe enlarges enormously and produces the sharp bend in the lesser curvature of the stomach, beside forcing the stomach downward and to the left. Again, at another time of life, the right lobe of the liver atrophies so rapidly that the pylorus is very much dragged over to the right side, thus increasing the apparent transverse position of the stomach. Yet this transverse position of the stomach is more apparent than real, for the line from the lower end of the esophagus is almost straight and parallel to the vertebral column. The delusiveness of the stomach position consists in the excessive bulging of the great curvature. But why there is such an excessive length of omentum is not clear. An explanation which I offer is that the force (liver) which produces the elongation of the mesogaster works continually on its base from beginning to end of formation. (Fig. 19.) The only method of explanation of the origin and method of formation of the great omentum must be sought through evolutionary development of the mammalian close connection with the condition and size of the liver. For example, Treves notes that in

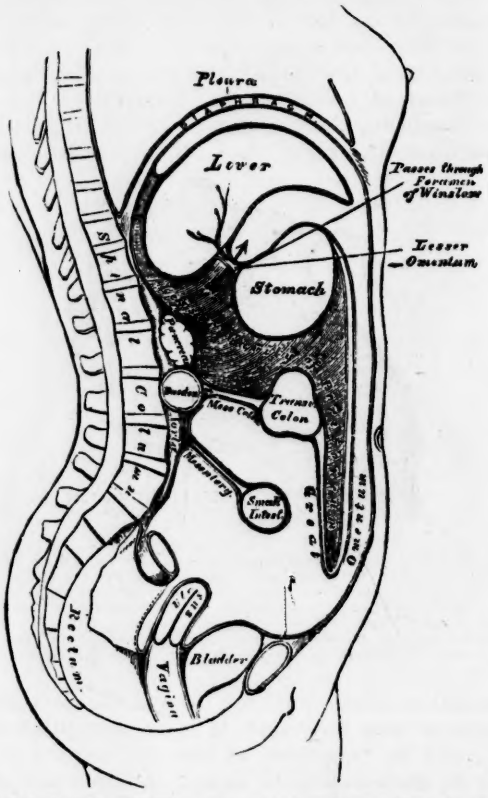


Fig. 21 (after Gray, 1887) represents a profile outline of the peritoneum as it lines the abdominal cavity. It is quite diagrammatic. The coccyx is drawn entirely too low, as it should only reach the level of the junction of the lower and middle third of the pubic bone. Notice the colon lies between the layers of the posterior ascending omentum, i.e., between the mesocolon. Neither the displacement or coalescence view is here hinted at.

the rudimentary disposition of the great omentum in the two-toed anteater a mere shallow bag is observed bending toward the left, but he does not say a word as to the condition of the liver, or why the anteater has such a simple mesogaster. Unfortunately I am not situated as Treves is, whereby his relations to the London Zoological Gardens enable him to examine the viscera of many animal species. In lower mammalia the mesogaster is a mere bag bulging toward the left with no relations with the mesocolon. The bag is a mere depression in the plane of the mesogaster and it can scarcely be considered to have a mouth or foramen. With the progress of

the mesogaster, bulging toward the left assumes more and more the shape of a bag lying around the great left bulge of the stomach. The wide circumference of the depression gradually contracts into a neck and, finally, jug-shaped, we have a bag (lesser omental cavity) and a mouth (Winslow's foramen). Now, the student would naturally suppose that the mouth of this bag formed by the elongated mesogaster would be at its mesenteric root, *i. e.*, on the vertebral column, but the atrophying right liver lobe has dragged the pylorus and lower end of the stomach to the right so that the neck of the sac or foramen of Winslow is also drawn to the right. But the relation of the foramen with the portal vein, hepatic duct and artery are not altered, for the whole apparatus, constricting the mouth, with the foramen, is dragged to the right. Hence the mouth of the sac (Winslow's foramen) is not situated at the root of the mesogaster on the vertebral column, but to the right where it is forced by the atrophying right liver lobe. An excellent diagrammatic illustration of the great omentum I have copied from the anatomy of Dr. Debierre which is from the valuable labors of Dr. Forabeuf. (Fig. 19). The involvement of the great omentum is inevitable where the rotation of the navel loop is so extensive as in man and higher mammals, because the right end of the transverse colon and the duodenum is so closely related.

All serous cavities are secondary. No serous cavity arises as a primary structure. Embryologic development teaches this fact in all animal life. Serous cavities are really points; points of motion. They regulate pressure, facilitate motion, modify circulation and allow visceral rhythm to have anatomic and physiologic functions. With the above views the anatomy and physiology and method of origin of the great omentum must be associated.

It is well to look at the root of the mesogaster for proof of origin and distribution of the great omentum. By examining the root of the mesentery it will be observed that the celiac vessel springs from the aorta at the middle of its origin. This short arterial trunk soon divides into three smaller arteries, viz., gastric, splenic and hepatic. A knowledge of the distribution of these three arteries is essential in discussing the mesogaster. The gastric artery starting from the celiac artery passes forward and upward to gain the cardiac end of the stomach. In its course it projects a fold of the right blade of the mesogaster forward for one to two inches. This fold is called ligamentum gastro-pancreaticum and it partially divides the lesser omental cavity into two compartments. The right compartment of the lesser omental cavity we may call the bursa omentum minoris, and the left bursa omentum majoris. The aperture between the right and left divisions of the lesser sac is the foramen omenti majoris while Winslow's aperture is the foramen omenti minoris. The fold of peritoneum thrown up by the gastric artery varies much in size and length. Generally it is one and a half inches high and it reaches from the upper border of the pancreas to the junction of the esophagus and stomach where the gastric artery gains access to the stomach. In some cadavers I observed that the gastric vein would accompany the gastric artery intimately for three-fourths of its distance and then diverge from the artery and border of the fold. Generally the gastric vein does not accompany the gastric artery very far in the border of the fold. The ligamentum gastro-pancreaticum is due to the gastric artery and not to the gastric vein in the vast majority of cases. The peritoneal duplicature produced by the gastric vessels projects from the upper and posterior surface of the omental sac, dividing the cavity into a smaller right cavity, containing a viscus, and a large left cavity containing no viscus. In quite a number of bodies I found another peritoneal duplicature projecting into the omental cavity beside the one due to the gastric vessels. However, they were small and inconstant, due to small blood vessels or to a past peritonitis. A practical method, to observe the peritoneal fold due to the gastric artery, is to well inject the arteries of a very spare subject and then to allow the subject to stiffen with cold, which will give a fixation and solidity to the viscera, so that their outlines and the borders of the peritoneal fold can be easily noted.

The hepatic artery which also springs from the celiac axis turns downward and runs under the right blade of the mesogaster until it reaches the ligamentum hepato-duodenal whence to gain access it turns and passes the liver. The hepatic artery passes directly downward from the celiac axis and slightly deviates to the right for several inches before it gains access to the hepato-duodenal ligament. During this whole course, from the celiac axis to its turn around Winslow's foramen, it generally does not raise the right blade of the mesogaster into a fold thicker than the width of the artery. One can easily observe the ridge produced by the peritoneal duplicature under which lies the hepatic artery. I never saw the ridge high enough to pass the point of the forceps between the artery and the anterior abdominal wall. As soon as the artery reaches the duodenal ligament it sharply bends forward, surrounding the foramen of Winslow and then producing peritoneal duplicatures. But after the artery has passed into the ligamentum hepato-duodenale it is beyond the pale of the mesogaster, so we drop its course at this junction. At this point the hepatic artery is important as it really forms the mouth of the sac known as the lesser omental sac. Though the distal end of the hepatic artery makes a sharp bend and hooks itself around a small aperture, hiatus Winslowi, yet the main trunk borders a larger foramen just to the left of the small one of Winslow, so that a careful examination on a well-prepared subject, fitted for the inspection, will reveal that an aperture exists to the left of Winslow's which is really a second Winslow's foramen. The aperture (foramen omentalis bursa) is bounded by the gastric artery in front and by the hepatic artery behind. It produces a constricted communication between the larger and smaller cavities of the lesser omental bag. We really have two foramina connected with the lesser omental bag. The first, foramen Winslowii primus, is the small end lying between the distal end of the hepatic artery and the inferior vena cava, and it leads into an anteroom (antrium bursa omentalis, or bursa omenti minoris) which contains Spigel's lobe. This anteroom of the lesser omental cavity is about the size of the fist, and is especially closed in front by the pars flaccida or middle portion of the ligamentum gastro-hepaticum. The second, foramen Winslowii secundiss, is a constricted orifice, bounded by the gastric artery and trunk of hepatic artery or rather the peritoneal duplicatures produced by the vessels. The foramen

leads from the anteroom or right cavity to the larger sac (bursa omenti minoris) of the lesser omental cavity. This left cavity is more uncertain and irregular than the right, as toward the spleen above and the ligamentum phrenico-colicum there are frequent constrictions due to past peritonitis. Various spaces and sacs are constricted off at the left end of the lesser omental cavity.

(To be continued.)

SOCIETY PROCEEDINGS.

Chicago Pathological Society.

The regular monthly meeting, March 11, 1895.

DR. HOMER M. THOMAS, Temporary Chairman.

DR. BISHOP exhibited for Dr. John B. Hamilton the specimens from a case of extra-uterine pregnancy, and prefaced his remarks as follows: I regret that he is not here to give us the clinical history and the points connected with the operative procedure. The notes from which I shall presently read were furnished by the interne in charge of St. Joseph's Hospital where the operation was performed. Dr. Bishop then read the following clinical notes:

EXTRA-UTERINE PREGNANCY—DR. HAMILTON'S CASE.

Clinical notes obtained from records of St. Joseph's Hospital.

Mrs. G. H., age 31 years, housewife. Family history: father died of consumption, mother suffering from the same disease. Personal history: always in fairly good health except for some injury in sacral region eight years ago, followed by an abscess in the pelvis, which was opened and drained through the vagina. Entire recovery after four weeks. Uniformly good health continued until about two years ago, when she was ill for a short time, having a vaginal discharge, her physician telling her she had miscarried, the fetus being only a few weeks old. Her present illness began one year ago with pain in the right inguinal region. Menstruation became scanty. Amenorrhea for past fourteen weeks. About two months before entrance to the hospital, the pain in the right inguinal region became very severe and has persisted ever since. Four weeks later profuse uterine hemorrhage set in, continuing up to five days before operation.

The patient consulted Dr. Hamilton Jan. 10, 1895, who advised laparotomy. Operation January 15. The right ovary and tube with the hematoma and contained fetus were removed. The remainder of the clinical history deals with the progress of the patient, which was uniformly favorable, she leaving the hospital February 3.

It will be seen that the ovary itself is fibrous, and there are a few cysts upon its surface. The clot altogether is not larger than a hen's egg and the sac in which the fetus lay is demonstrable at the outer end of the tube which has been incised and held open. As will be seen by examination of the specimen this is an example of tubal gestation. The outer end of the Fallopian tube is expanded to the size named, by the clot, and the fimbriæ can be seen coursing up over the extremity. It would seem that the implantation of the ovum took place very near the fimbriated end of the tube. The tube and clot were incised before reaching me; save for the incision the tube walls appear to be intact. The fetus (shown in a separate bottle) is about 2 centimeters long, probably four or five weeks old.

DISCUSSION.

DR. GEORGE HENRY CLEVELAND—I am sorry Dr. Hamilton is not present this evening, as I would like to ask him one or two questions, one of which is as to whether the diagnosis of extra-uterine pregnancy was made previous to operation or not.

DR. BISHOP—The notes make no statement on that point, and I have had no conversation with Dr. Hamilton.

DR. CLEVELAND—The question of diagnosis of extra-uterine pregnancy is of a good deal of interest. Tait has created the opinion, either by statement or by his practice, that it was next to impossible to diagnose a case of extra-uterine pregnancy previous to rupture, but that after rupture it was an exceedingly simple thing. Dr. Noble, of Philadelphia, does not agree with Tait, and I have lately observed that he reports some twenty-five cases, in the *American Gynæcological and Obstetrical Journal*, of extra-uterine pregnancy, in nineteen of which a positive diagnosis was made. Noble says: "Of the cases in which a diagnosis was made (one strongly suspected) three of them were instances of unruptured tubal pregnancy, and seventeen (two strongly suspected) of cases in which rupture had taken place."

There is another exceedingly important and interesting point in connection with the subject, viz., the causation of tubal pregnancy. In looking over the subject I find it stated that the generally accepted opinion now is that the place of contact of the germ and sperm cell is always in the body of the uterus, and that subsequently the fecundated ovule passes into the tube, lodges there, and then develops. It is a pretty theory, if it is a tenable one.

DR. E. R. LE COUNT—I would like to ask Dr. Bishop whether the case was one of tubo-ovarian pregnancy? I did not exactly catch the description of the case so as to differentiate it.

DR. BISHOP—The specimen plainly shows that there was no relation with the ovary in this case. The ovary is remote from the tube, and was in that position when I received the specimen.

MICROSCOPIC SLIDES.

DR. HUGH T. PATRICK, in demonstrating microscopic slides, illustrating some of the more recent methods of staining nerve tissue, said in substance: The anatomist, histologist, and pathologist will probably agree that the nervous system constitutes the most difficult part of the field of their labors, notwithstanding the fact that enormous strides have been made in our knowledge of this system in the last fifteen or twenty years. As we follow the steps of this advance, we are forcibly struck by the fact that each step was due in large measure to some improvement in appliances or technique. For this reason it is essential that those who would keep abreast of the times and wish to do practical work in the anatomy, histology or pathology of the nervous system must be acquainted with the more recent methods of investigation.

Slides illustrating the following methods were exhibited:

Kronthal's method which is particularly adapted for demonstrating the larger cells of the nervous system.

The Bevan Lewis stain is especially important for the cerebral cortex, and it is claimed for it that it shows details and changes not discoverable by any other method. It is particularly valuable in showing hypertrophied neuroglia cells. For the nerve cells the speaker considered it inferior to the Nissl stain which shows the structural details of the various ganglion cells with most exquisite distinctness. Nissl has gone so far as to make a classification of nerve cells based upon the findings by means of this method.

The Rosin stain is simply a modification of Ehrlich's blood stain, and the author would classify the cells of the nervous system on the same basis as Ehrlich divides the various blood corpuscles. His views have been fiercely attacked by Nissl.

The silver stain (Golgi, Ramon y Cajal,) has given us most of the later important discoveries in the structure of the nervous system with respect to both cells, fibers and the various systems. It is not adapted for pathologic work.

The chlorid of iron, dinitroresorcin stain, introduced by Platner, is particularly valuable for peripheral nerves. The speaker hoped to report more fully upon this method in the near future.

The Marchi method is adapted for following the degeneration of fibers not exceeding three months in duration, and in these cases it gives pictures excelling in accuracy and distinctness those of all other methods.

Slides were also shown, demonstrating the Flechsig developmental or embryonic method, the new Weigert neuroglia stain, and peculiar results sometimes obtained by the Weigert-Pal method.

DR. H. B. STEHMAN reported two cases, one of

SUBMUCOUS FIBROID OF THE UTERUS,

and the other of

CARCINOMA OF THE RECTUM,

and exhibited specimens. Mrs. C. J. V., age 55; the mother

of several children has never had any uterine trouble, and passed through menopause without any special disturbance. Two years ago she began to have an occasional uterine hemorrhage; these in time became not only very profuse, but she scarcely, of late, was free from them for any length of time. Upon examination a large tumor occupying the entire uterine cavity, attached to the left and entire fundus, was discovered. The cervix was split bilaterally and enucleated with great difficulty owing to its size. The uterine cavity was tamponed with iodoform gauze and the cervix repaired. Owing to careful aseptic precautions there was no febrile disturbance and the patient was up and about in ten days.

A striking feature of the tumor is not only its form but the depressions which make its resemblance to a fetal head all the more striking. Deep perineal stitches were then used to approximate the wound, leaving an opening of about one inch at the upper angle, which was partially packed with iodoform gauze, the same extending around a tube which communicated with the bowel. This dressing remained unchanged for forty-eight hours, at which time both tube and gauze were removed and re-inserted. The deep perineal sutures were left in ten days, when it was found that union was complete throughout. Several of the vaginal stitches sloughed, causing a fistula which remained open for several weeks, but under faithful dressing closed.

The external wound, which was left for an anus, showed a disposition to close, and a hard rubber tube attached to a plate, so constructed that it would barely reach the bowel, but sufficiently curved to fit it nicely, and held by soft rubber tubing, overcame this disposition and left the opening that had properly granulated. The tube is now worn at night, and the patient is quite delighted with her physical condition which has vastly improved since the operation, and also with the control she has over the fecal movements, provided the feces are not too liquid.

Miss M., age 65. About a year previous to the operation she began to feel some soreness in the rectum, which became in time quite distressing during the act of defecation. Accompanying the movements there was considerable mucopurulent discharge which was occasionally mixed with blood. On digital examination the rectum was found ulcerated, more particularly on the anterior surface and extending up about four inches on the left side. The infiltration involved not only the rectum but the cellular tissue separating it from the vagina. A modification of the Kraske operation was performed, removing the coccyx with the lower sacral vertebra. As you will notice by the specimen, the entire rectum was resected, the dissection including the sphincter ani, the perirectal fat *in situ* and the posterior wall of the vagina up to the fornix, while the bowel itself was amputated still beyond this point. The specimen has been in absolute alcohol for three months, and is quite shrunken. The vagina was then closed with the stitches on the mucous surface, leaving a canal of about one-half inch caliber.

Dr. E. J. Senn read a paper entitled

REPORT OF A CASE OF OSTEOMYELITIS OF THE ANKLE-JOINT, ILLUSTRATING THE UTILITY OF BONE CHIP IMPLANTATION.

The patient was 18 years of age, female, admitted to St. Joseph's Hospital with disease of ankle-joint. The case was of two years' duration. Diagnosis of chronic suppurative osteomyelitis resulting from a previous acute osteomyelitis. Patient was etherized, incision made over sinus into joint cavity. A sequestrum in lower end of tibia was removed and some superficial necrotic bone in the astragalus was dealt with by curettement. Cavity was disinfected with mercuric chlorid solution and hydrogen peroxid, dusted with iodoform and packed with decalcified bone chip. Recovery was uninterrupted, the patient bearing entire weight on foot in seven weeks. A point of interest in the case was that probably the osteomyelitis had its origin in the tibia above the epiphysis, and instead of the pus taking the usual course and discharging on the surface after penetrating the wall of the tibia, it extended to the epiphysis and emptied into the joint.

DISCUSSION.

Dr. H. B. Stehman—I do not propose to discuss Dr. Senn's paper, but simply to call attention to one point, and that is the method of securing asepsis. I would like to raise a protest against one thing that he has used, which is probably one of the most efficient remedies we have to secure asepsis under certain conditions, and that is peroxid of hydrogen. I believe I can substantiate what I say. While peroxid of hydrogen is one of the most efficient aseptic agents we have, it does more mischief than any other agent we use in a great many cases; that is, we have a more extensive septic infection after the use of it. I am not criticising the manner in which the agent was used in this case, but I would call the attention of the members of the society to its dangers. Not very long ago I saw a case of death from the use of peroxid of hydrogen, and it is a very dangerous agent to use where we have not free drainage and where there is a great amount of cellular tissue. It is dangerous to use in tendons, as it travels along tendon sheaths and carries infection. I am satisfied I have seen infection carried up the arm where peroxid of hydrogen has been used in infection of the fingers. I simply ask you to beware of it.

The President—Dr. Stehman's experience is perhaps a little remarkable. It may not coincide with the experience of others.

Dr. Joseph B. DeLee—The subject of the use of artificial means to procure obliteration of cavities is a very interesting one, and the use of ivory for this purpose instead of decalcified bone chips is now being worked upon by Professor Gluck, of Berlin. He takes discs of ivory an inch and a half in size and replaces defects with them, and in the active process which goes on the ivory becomes a part of the bone. I have seen dogs operated on in this way and in three or four weeks they would be jumping around the room as if nothing had happened to their femurs. He has taken a joint made of ivory, placed it in position with the normal joint, fastened it, and the joint has become active. This has been demonstrated on the human being. In one case in which the third phalangeal bone of the second finger was missing, by means of two platinum pins he fastened the phalanx, which was made of ivory, and the patient is moving the finger to-day as if he had the normal bone in it.

I think there is a large field open to us in the replacement of defects by means of artificial substances. Gluck has taken steel pins to replace defects in bone, which become so walled in by the tissues, and especially bone, that they will remain for years as a part of the body. In some sections of the joints that were made with ivory, it was not an easy matter to determine where the ivory stopped and where the bone began. The subject has not been as well worked up in this country as it should be.

Dr. George H. Weaver—There was one remark made by Dr. Senn which particularly attracted my attention, and was to this effect, that probably the space at the time of operation was not perfectly aseptic. It has been demonstrated—as I presume most of you know—that it is not necessary to make the empty spaces absolutely aseptic. A series of experiments were made by Halsted, of Baltimore, in which he allowed large empty spaces to fill with blood and introduced a considerable quantity of pyogenic organisms into the cavity, finding that these were destroyed and taken care of; that the space was filled with granulation tissue, replacing the temporary blood clot that was formed. If the empty space is filled with various materials in this manner, the secretion from the wound following the operation will take care of a certain number of bacteria if they are not too numerous and of too great a degree of virulence.

Dr. E. J. Senn (closing the discussion)—I wish to say in reply to Dr. Stehman that in certain cases peroxid of hydrogen is harmful, that is, in those cases in which we have a circumscribed focus of suppuration, because the pressure due to the decomposition of peroxid of hydrogen will increase the pressure within the abscess cavity, and disseminate microbes into the connective tissue spaces, and from here enter the lymphatics, eventually becoming diffused. In a case like the one which I have described, the whole joint cavity was infected, and there was very little danger of the infection becoming more diffused by the use of peroxid of hydrogen. I would ask Dr. DeLee whether the ivory joint he mentions was expected to remain as a perfect joint. I was always under the impression that ivory became absorbed sooner or later, but not as readily as catgut. In the cases which I have seen, where ivory bone nails were used in ununited fracture, they eventually became absorbed, and I should think that ivory used in a joint would also in the course of time be absorbed.

Dr. DeLee—The part of the ivory that had become united was infiltrated by the bone cells, so that it is possible in the course of time that the ivory will be entirely replaced by bone and perform the functions of a natural joint. In the case I have reference to, the ivory joint remained for several months, finally suppuration took place and it had to be taken out. The wound was entirely united and we could hardly tell where the bone ended and the ivory began.

American Electro-Therapeutic Association.

Fourth Annual Meeting held in New York Academy of Medicine New York, Sept. 25, 26, and 27, 1894.

WILLIAM J. HERDMAN, M.D., President.

(*Continued from page 70.*)

DR. W. S. HEDLEY, of Brighton, Eng., read a paper on

THE HYDRO-ELECTRIC THERAPEUTICS OF THE CONSTANT CURRENT.

This paper chiefly concerns itself with the electric water bath. Those localized applications by douche, by water introduced into cavities, or by local immersions are, it is understood, to be dealt with in other communications. To designate respectively that form of bath in which the water constitutes only one pole of the circuit or that form in which both poles are in the water the terms "monopolar" and "dipolar" are adhered to. It seems scarcely necessary, however, to admit the words "multipolar" of recent introduction. To regard the multiplication of electrodes as a divided anode or a divided kathode is probably simpler and certainly more correct. The question of exact dosage in the dipolar bath is one of pure physics. In other words the following is the problem that presents itself: with a given current running through a given bath what proportion of such current will find its way through the body of a patient immersed therein? This question, however, does not enter into the scope of the present paper. It is one of extreme difficulty, and well worthy the attention of those distinguished physicists who are about to discuss it. One of the chief difficulties lies in ascertaining body resistance under the conditions of immersion. It is obvious that the body then becomes part of a compound conductor and that it will take its share of current according to definite physical laws. As to what that share is, various estimates have been hazarded. These have varied between 5 per cent. of total current flowing and 20 per cent. and within an even wider range. But so far as the present writer is aware, neither by direct measurement of body resistance nor by more indirect methods has this question ever received a satisfactory answer. Under these circumstances therefore dosage can only be expressed in terms of a certain current, running through a certain bath, containing a human body under certain conditions of immersion. It also becomes evident that if results are to be compared and any approach to exactness arrived at, in no form of electrization is "standard" apparatus of greater consequence than in the case of the electric bath. The dimensions and construction of the bath tub which the writer has found useful in practice are given in an appendix to this paper. With so large a number of electrodes the current is easily manipulated by means of a suitable switchboard. This subdivision of the two poles presents advantages, both in the way of diffusion and localization. Until after investigations more extended than any that have yet been carried out, it will be difficult to assign to the constant current bath its exact place in therapeutics. So far as the present writer is concerned it is still on its trial. He does not hold a brief in its favor. Indeed so far from being its apologist he has to confess that he now finds himself using it much less frequently than the bath with sinusoidal and other alternating currents. But at the same time he feels bound to ask himself how far his practice is based on careful inquiry, and how far it is a mere experiment; how far carried away by the enthusiasm of others; how far borne along on one of these irresistible fashion-waves that every now and then in medicine as in other things sweep everything before them. It may at least be well to suspend judgment until some of the excitement has passed over which, for the moment at least, seems to have placed alternating currents everywhere in the ascendant.

Before proceeding to details, it is well in the first instance to clear the ground by distinguishing between the electric and the purely thermal effects of a bath. The warm bath (*i.e.* the bath at 95 to 104 degrees F.) slightly increases the activity of the circulation and somewhat retards tissue metamorphosis. It is a valuable soothing agent, but in other respects its effects on the system are almost inappreciable (Shelly). It appears therefore that we have not much to expect from the warm bath *per se*. As a means, however, of applying electricity to the body it is invaluable.

With the dipolar form of bath and a bath tub of the construction and dimensions indicated[1] the writer finds that he uses a current strength of 50 to 280 milliampères. With the cervical, dorsal and terminal electrodes in action the first bath begins at the lower figure (the current being *gradually* turned on through a cell collector or rheostat) for the first five minutes. A somewhat greater strength is gradually attained during the rest of the time—which altogether extends to eight or ten minutes for the first bath. Both duration, direction and strength will vary according to the therapeutic result aimed at, the strength and electrical capacity of the patient, the number of electrodes in action and other circumstances. The case is then watched for a day or two, and another bath given with an increasing current. In another day or two, a third bath is administered. Tolerance having thus been attained and no contra-indication having manifested itself, a series of half a dozen baths are taken in successive days. Then follow two or three on alternate days; an average "course" thus extending over about three weeks and consisting of twelve baths. The position of the patient in the bath will vary according to circumstances—completely immersed if desirable to act on the whole length of the spine or sitting upright on the gluteal electrode in the direct line of current between the two laterals in cases where it may be intended to influence the hips or the organs of the abdomen and pelvis; the arms being allowed to lie across the line of current flow or carried to the front of the patient so as to avoid it. Temperature will vary according to circumstances, but 94 to 100 degrees F. is a sufficiently wide range in this climate. It is often considered useful to allow a faradic current of moderate strength to run for a few minutes before the patient emerges from the bath, or while he stands up in the water a cold affusion in some form or a mild faradic douche is applied, according to individual toleration. The patient who finds it necessary to undergo a course of electric baths has not generally any considerable resisting power to cold; he is moreover often nervous or hyper-sensitive. Therefore if stimulation either by cold affusion or by the electrized douche be attempted it is obvious that the operator must have his apparatus well in hand. It is scarcely too much to say that the success of the electric douche depends upon the easy graduation and the perfect controllability both of its electric and of its hydriatic strength. A quarter of an hour's rest is taken and weather and strength permitting, the patient walks at least part of the distance home. In administering a bath to insane, paralyzed, anesthetic and other helpless people, special precautions must be taken especially if the patient be heavy as well as helpless. In certain cases, paralyzed in the lower extremities only, I have availed myself of the suspension tripod ordinarily used for locomotor ataxy. With this placed across one upper corner of a low bed and with an improvised seat or "chair" made of straps or stout webbing, a patient can easily be swung into a bath placed alongside the bed as seen in this rough sketch.

If wholly helpless, the apparatus figured and described by Mr. Stephen for the cold bath treatment of typhoid may be used. Of this, also, I attach a very rough reproduction.

In all such cases the greatest care must be exercised, not only in placing the patient in the bath, but in looking for wounds, ulcerated surfaces, skin abrasions, etc., and protecting them from the action of the current.

Before being placed in the bath the condition of the patient's "sensibilities" (tactile—pain—temperature—muscular sense) must be carefully inquired into. It is within the writer's knowledge that in the case of tabetics who were also anesthetic, serious ill effects have followed the administration of electric baths by incompetent persons whose only limit to dosage is what the patient will bear.

The question of polarity will have to be decided on general principles, or rather by a process of cautious experimentation. The late Dr. Stevenson states that the direction of current flow should generally be from the feet to the head. No reason seems to be assigned for this rule and it is not altogether easy to feel satisfied of its usefulness; but always necessary to bear it in mind, as being the opinion of an admitted authority and the outcome of a large experience.

Although the douche is the subject of a special paper, it will not be out of place to advert to it here as a means of applying the constant current for the purpose of general electrization. To avoid the necessity for repetition and to secure at the same time a certain amount of completeness, some details of apparatus with explanatory remarks thereon, already published by the writer, are added as an appendix to this paper.

Rules for administration, intensity—direction and duration of current flow—the necessity for a *gradual* making and breaking of circuit (unless when the special effects of sudden rupture or reversal are desired), all these things are as in ordinary electrical applications and need not be entered

[1] In Appendix.

upon before a society like this. It will be noticed that the douche apparatus is *single*, not double. The latter has recently been recommended and the details of application as given in a recent essay[2] are as follows: there are two douches, or jets, one attached to each pole of the battery. While one is directed upon the body of the patient, the other is directed downward on the cemented floor of the cabinet, until a sheet of water is formed surrounding the patient's feet, thus constituting the second or indifferent electrode. This seems a very unnecessary application of douche to the floor, and it is difficult to see why the second electrode be not in metallic communication with the source of supply. There are circumstances when two jets might be useful to secure a strict localization, but any one who has tried to manipulate two jets held at a definite distance apart, and both at equal distances from the body, and at the same time to watch the galvanometer, will not be slow to express a preference for a purely polar applicatiion and an "indifferent" electrode. Those who are familiar with what has been already written on this subject will not materially add to their knowledge by a perusal of this essay unless, indeed, it be by the following electrical discovery. Its writer finds to his surprise that not only water but watery vapor will conduct his currents; and the reason why he thinks so is this: the wire connecting one pole of the induction apparatus to douche No. 1 having been detached from the latter, rolled up and hung

about 3 meters from the patient (its other extremity being still connected with the coil), and the second wire still attached to the second pole of the induction apparatus as well as to the metallic part of the second douche, and the cabinet full of a thick cloud of watery vapor—behold, a current was established. The patient said he felt it and the fact, it is stated, was further verified by a galvanometer and a constant current. "Evidently, therefore, the circuit was completed by the watery vapor surrounding the body of the patient."

Before publishing this conclusion it might have been well if the writer, leaving behind him his douches and cabinets and moisture-laden air—but taking with him a large coil and (say) two bichromate cells to drive it, had proceeded to a dry room and there carried out the following experiment: attaching a conducting wire by one of its ends to the secondary of the coil, the other end of this wire being free and the other pole of the coil being "idle," let the coil be put into action. If now the free end of the wire be touched by the finger, a current is felt to pass each time contact is made. A person slightly insulated might then take into his hand an ordinary electrode attached by its rheophore to one (secondary) pole of the coil. If now the "operator" lightly touch the person's forehead a distinct electrical sensation is felt; other things being as in the last experiment—the "idle" pole of the coil might be connected with a gas or water pipe. Then if the arm be touched by a second person standing "to earth," a distinct effect just short of muscular contraction will be produced. These are not mere experiments, but modified in a variety of ways they are most useful in practice. But the present point is this, that in all these cases, there is a circuit completed—one pole is "idle" and—where is the moisture-laden air? A few experiments with this "idle pole" work might have suggested to the writer in question, that when he hangs up his wire or carbon plate attached to one pole of the coil, he is perhaps in reality only making a good "earth" for his idle pole—via ceiling, wall and floor, and that the earth rather than the watery vapor is the medium of conduction. But there is the control experiment to deal with. It is stated that when a galvanic battery was substituted for the coil, a galvanometer in circuit showed a deflection. Nothing is stated as to the character of this deflection. Was it a steady and persistent rise denoting leakage—or was it a "kick" denoting some possible capacity effect, or was it neither, or only an error of observation? Be the explanation what it may, the following simple experiment will exclude the one (watery vapor conduction) offered in the essay referred to. Let the experimenter provide himself with a tea-kettle boiling and with a

good jet of steam issuing from the spout. Then taking a metal tube say two inches long by two inches diameter, connect it with one pole of a c.c. circuit having a pressure of 115 volts, placing in circuit a galvanometer, and a rheophore of (say) 600 ohms. Hang this tube at the end of the kettle spout so that the steam issuing from the spout becomes condensed in the metal tube and assumes the appearance of a dense cloud of watery vapor. To the other pole of the source of supply, attach by its rheophore an ordinary metal disc electrode having a wooden handle. Now, holding this handle, let the watery vapor driven through the tube play upon the metal disc held at say one-fourth of an inch or as near as possible to the tube. The ordinary galvanometer will not betray the completion of a circuit by the very faintest "kick." How, then, can it be hoped to convey a therapeutic current by such means? Now all this is very elementary and I owe an apology for bringing it before this society. But the case quoted is no solitary instance of that loose and inaccurate work which has again and again brought medical electricity into disrepute. Better than to theorize is simply to *try*.

Speaking broadly, the painless and evenly distributed current of the electric bath makes it one of the best methods of general electrization, with, at the same time, a considerable power of concentration on special parts, according to the indications of the case. In all states of general debility

[2] Sur une nouvelle méthode d'application des courants electriques a l'aide de l'eau et de la vapeur d'eau. Par le doctor P. Guyénot.

and impaired nutrition (Erb), in weakness or exhaustion of the spinal nervous system, "nervous dyspepsia," palpitation hysteria (Erb), neurasthenia, ("nervous breakdown,") and many of those diseases referable to some derangement of the nervous system without appreciable lesion, commonly called neuroses, it may be resorted to with excellent and unique results. Neuralgias, sciatica (whether perineuritic or purely neuralgia), paralyses both of central and peripheral origin, chorea, primary lateral sclerosis, muscular rheumatism, gout, rheumatoid arthritis, and occasionally chronic articular rheumatism are all recorded to have been cured or alleviated by its use. It is said that it has been used with good effect in some irritative conditions of the spinal cord, in alcoholic or mercurial tremors, plumbism, and even paralysis agitans (Lehr, Erb), and peripheral neuritis from whatever cause, though not, perhaps, in every stage.

Of the cases which the present writer has found himself able to treat by the constant current bath with a fair measure of success, perhaps the largest is that of rheumatoid arthritis. He, comparatively seldom, is fortunate enough to encounter this class of case in an early stage when it can be dealt with as a trophoneurosis—but even when of old standing with articular mischief well established, locomotion may be improved and pain relieved more frequently, at least, by this than by any other remedial measure with which the writer is acquainted—with the exception perhaps of baths with the current from alternating light circuits, having a pressure of 100 volts and about 8,000 alternations. His experiences of this treatment in other diseases may be summarized thus: muscular rheumatism frequent and rapid cures—not superior, however, to other electrical methods. Chronic articular rheumatism—results on the whole disappointing. Gout often improved after a course of baths in suitable cases, but being generally combined with other remedies and *always* with dieting, the exact relationship of cause and effect becomes comparatively obscure. Sciatica, good results—not superior to alternating current baths but perhaps superior in the majority of cases to other electrical methods. Urethral synovitis only yields to these baths when the disease has continued for some time and is probably waning in the ordinary course of events. Locomotor ataxy—occasional alleviation in symptoms but not always easy to separate results from those periods of arrest which usually characterize the disease. Not all cases suitable. Chorea —ordinary cases sometimes get rapidly well under a course of these baths as after faradic baths or any other form of treatment, or after no treatment at all. But that comparatively small class of cases (occurring before 20) which not only do not cure themselves but often resist even rest and arsenic, and known as "relapsing" chorea have been treated with only temporary benefit, lasting perhaps for only a few hours after each immersion, seldom with a permanent success. In amenorrhea, a current localized between the lateral and gluteal electrodes has in several instances been found quickly successful. Among the rarer cases one of melancholia and one of hysterical paralysis have been greatly improved. In the insomnia of neurasthenia this form of bath has been found useful as a beginning of electrical treatment. A case of "ataxic paraplegia" has been notably benefited and in Parkinson's disease the tremor temporarily alleviated. In the four last-mentioned cases, however, alternating currents were also used.

The following result of treatment by electric baths has been kindly sent me by Dr. Lewis Jones from recent records of the Electrical Department of St. Bartholomew's Hospital:

A—GALVANIC BATH, ANODE TO FOOT.

Sciatica:

1. Louisa F., 41, twelve baths, 200 milliampères, sciatica, cured, general health improved much.
2. Louisa F., 41, eight baths, cure.
3. Henry G., 64, relieved after four baths; returned five months later; eight baths, but no relief.
4. Henry G., 64, cure after four weeks; eight baths.
5. M., gonorrheal rheumatism, nine baths, no definite improvement.
6. F., rheumatoid arthritis, twelve baths, slight improvement.

B.—FARADIC BATH.

7. H. B., male, 43, 7 baths; induction coil; cure.
8. Male, 47, seven baths; induction coil; cure. A long previous treatment with anode to thigh (without baths) ineffectual.
9. F., 50, rapid cure, number of baths not given.
10. Thos. H., 25, galvanic baths first, then anode to thigh. Then faradic baths last, most useful—relieved.

C.—ALTERNATING MAINS.

11. R. E., 60, male, fifteen baths, sinusoidal, cure.
12. F., 30, female, four baths, sinusoidal, cure.
13. F., 30, female, same patient with general rheumatic pains two years previously, had galvanic baths, twelve, and was cured.
14. W. H., 23, male, five baths, much relieved. Left London; wrote a week later that improvement continued.
15. G. T., 25, female, severe neuralgia (cervico-brachial), ten baths, sinusoidal, cure—old standing case.
16. F., rheumatoid arthritis, twenty baths, no improvement.
17. F., rheumatoid arthritis, twenty baths, doubtful.

Those rarer uses of the constant current bath consisting of the introduction of medicinal substances, or the elimination of metallic impurities ("medication and de-medication") must be considered. The latter is demonstrable and certain. Apart from other evidence, a case has occurred within the writer's knowledge where an appreciable quantity of lead which could have come from no other source than the tissues of the body, was found in the deposit on the copper plate. To carry out the process in a case of plumbism, a little sulphuric acid is added to the water of the bath into which a large copper plate connected with the negative pole of the battery is introduced.[3] The patient then grasps the anode (outside the water) and the current is gradually turned on. The lead from the patient's tissues becomes deposited on the copper plate according to the law well known to electro-platers. For metals other than lead, nitric acid takes the place of the sulphuric. The question of making use of the constant current bath as a means of introducing medicinal substances into the body deserves a close and patient investigation. Now that the process of cataphoresis has passed the experimental stage, this method is not without a promise of tangible results. Its practicability will depend upon the density of current necessary to secure with a given substance cataphoric transference through the skin. If, in order to secure a large surface for the anodal diffusion, the bath be made monopolar and the water anode, the density of current per square centimeter of body surface must be exceedingly small. If the external electrode be anode, it is difficult to see any advantage of the bath over ordinary methods of cataphoric medication. Speaking in the absence of experiment, which alone can decide such questions, it would seem that the dipolar bath, in which large currents are practicable, will be found the only efficient form of bath for this purpose. For use with mineral waters, and equally for medicated water, Gartner has, it appears, introduced a division or diaphragm into baths. The present writer has not seen this arrangement, but presuming that the body passes through the diaphragm and even without supposing the possibility of securing an actual electro-tight junction between the two, the arrangement seems practicable. (It may be suggested that (supposing an electro-tight junction), the same expedient might also perhaps be available to determine body resistance under conditions of immersion.) The questions, then, for the experimentalist seem these: What is the current density required to pass a certain substance in a solution of a certain strength through the skin? Is such a density theoretically secured on the surface of the body by the current proposed to be used? Can the medicinal substance be found in the secretions or excretions of the body after having been subjected to the conditions named?

The treatment of skin disease by electrical methods is one of great and growing importance. The material in possession of the present writer does not enable him to deal with it in any satisfactory way so far as the constant current bath is concerned. Judging, however, by the few cases in which he has seen it used (urticaria and eczema), and by the effects of other forms of electrical treatment with which all are familiar, this form of bath would seem to deserve a patient and extended trial. No doubt there are those present who have valuable experience on this point. For purposes of the present paper and his own information the writer has consulted special works on cutaneous disease, but without adding materially to his information. Among these works, however, there lies before him a brochure on "The Bath in Diseases of the Skin,"[4] wherein (at p. 81) he finds a passage so interesting and instructive that he ventures to extract it: "It will naturally be expected that I should say something of the galvanic baths. . . . . I frequently employ them

[3] Unless the bath itself be of copper.

[4] The Bath in Diseases of the Skin, by J. L. Milton, Senior Surgeon to St. John's Hospital for Diseases of the Skin; Lecturer on Diseases of the Skin; Member of the Harveian Society; Corresponding Member of the New York Dermatological Society, etc.

through the medium of a foot bath; one foot being placed on each handle and the force being given by the 100 cell Becker Muirhead used at the hospital, or a good-sized wheel magnet." After indicating this latter novel source of galvanic supply the author proceeds: "But to suppose that a galvanic bath possesses any power beside that inherent in galvanism . . . . I certainly say that if such be the case it has entirely escaped my powers of observation." So say we all, and it would be pleasant to leave the question at a point where none can differ, but the following passage seems too instructive to omit: "In matters of charlatanism an amount of credulity is constantly met with even among the educated classes of this country which would be considered abject superstition in a Hottentot. The opinion may seem a strong one but . . . . what are the proper words to use respecting the confidently expressed statements by patients that they know that the disease has been drawn out by the electricity and that they have seen it deposited at the bottom of the bath in the shape of shreds." To those who know anything of the matter an interesting question here suggests itself. Was this a case of metallic poisoning? Is it possible that a patient in describing the disease as "drawn out by the electricity," might have been making a practically accurate statement of what sometimes is known to occur? Is it within the bounds of possibility that this was an instance of the elimination of metallic impurities from the body by the agency of the constant current bath —a case of "de-medication?" Perhaps, after all, a patient without the learned author's medical (and electrical) knowledge in speaking of such a deposit had scarcely done anything so outrageous as to deserve to be called a Hottentot. We must, however, tear ourselves away from this fascinating and instructive volume; this paper has already grown to undue length and its writer owes and offers an apology.

APPENDIX.—(Not forming part of the foregoing paper, but explanatory of it and inserted here to secure a certain measure of completeness without the necessity of repetition.)[5]

Against the hydro-electric methods, more, perhaps, than against any other form of electrical treatment, has been levelled the charge of empiricism. And, perhaps, not without reason. Never having been adequately investigated, their real power is but little known, and their real province but little understood. Could anything, for example, be looser and more haphazard than the way in which that commonest and best of hydro-electric methods, the electric bath, is commonly prescribed and administered? For carrying out treatment by these methods, the electrical equipment must be not less complete than that for other purposes. The requirements will be:

1. A constant current supply in the shape of a battery with a low resistance which will work up to a powerful current strength through the estimated R. If we take our supply from an electric light circuit, it will have to be safe-guarded by shunts and appropriate resistances in addition to a reliable "cut-out."

2. A means of opening and closing the circuit, and regulating strength by easy gradation so as to avoid pain or shock, *i.e.*, "current-collector," or rheostat or, in dynamo circuits, an adjustable rheostat or a sliding shunt.

3. A milliampère meter, *i.e.*, a galvanometer graduated in milliampères, and registering up to, say, 500.

4. A powerful induction coil for faradic bath or supply from an alternating dynamo.

5. Some means of suddenly reversing the current ("current reverser" or "pole changer") as well as an arrangement for throwing the two currents together for combined use ("current combiner" or "De Watteville key").

6. As an adjunct but not an actual necessity may be mentioned a voltmeter, useful for occasionally determining the electro-motive force of the battery, or any particular cell, and for other purposes. Some galvanometers are also voltmeters.

The ordinary electric bath is best made of oak or porcelain, perhaps porcelain for choice, if expense need not be considered. Insulation must be carefully attended to, both of conducting wires and waste-pipe, the latter being insulated from earth by a short length of rubber tubing let in near the bath.

The bath may be an ovoid oak tub, four feet ten inches long and two feet six inches at greatest width, which is about two inches nearer head than foot. Height at head one foot eleven inches, height at foot one foot five inches. There are five fixed electrodes of bright metal covered only by light removable open wooden framework, size as follows:

[5] The Hydro-electric Methods in Medicine, W. S. Hedley, M.D., July 1, 1892.

Cervical, 28 x 29 cm.
Lumbar, 24 x 17 cm.
Lateral (2), 26.5 x 18 cm.
Gluteal (circular), 30 cm. (diameter).
Terminal (foot), 22 x 38 cm.

In addition to these there is an electrode for monopolar purposes, consisting of a removable metal rod, one inch in diameter, covered with wash leather. This is fixed across the widest part of the bath, and can be conveniently grasped by the hands. These electrodes are connected, by carefully insulated wires, with seven terminals, and these in turn lead to a switch-board, so arranged that by the insertion of plugs any electrode can be brought into action, either as anode or kathode. The connection with the battery, coil, or other source of supply, is by means of well-insulated connections leading to two ordinary "binding-posts" on the plug switch-board.

It is best in the interests of cleanliness, if for no other reason, that the electrodes be of bare bright metal, as the cleansing and changing of covers is often very imperfectly attended to, and is a matter of serious consequences sometimes, as several unfortunate instances show.

The size of the electrodes will depend upon the theoretical considerations already advanced, looked at in the light of experimental results which are detailed elsewhere.[6] But, speaking generally, the sizes and positions of those electrodes already mentioned are suitable. The only further addition (and a very useful one) is the so-called "paddle" electrode. By means of a long insulated handle this electrode can be applied to the vicinity of any part of the body upon which it may be desirable to concentrate the current.

For the comfort of the patient it is sometimes considered advisable to use some support for the shoulders. Individual views and ingenuity will here be the guide. One way of effecting the purpose is to use straps, one under the back of the neck, the other supporting the body under the arms. Another method is to have an open framework, made of wood and webbing, somewhat like the invalid appliance known as a "bed-rest." For spinal affections the wooden "back-rest" may be modified by having it made solid, excepting a longitudinal opening throughout its whole length opposite the vertebral column, the upper electrode being of such size, shape, and distance from the part as to allow the lines of current flow to concentrate themselves on this opening. My experience, however, is that with the bath shaped as described—the fixed metal electrodes in the positions named, and controlled by a switch-board—the current can be so handled as to concentrate it, or diffuse it, or send it longitudinally, or transversely, or obliquely, through the water as may be desired.

The question of "density," which is so important a factor in "dosage," and which in ordinary electrical applications depends upon the size of the electrodes, becomes a very complicated one in the dipolar bath. Here it is evident that not only the size of the electrodes is to be considered, but also the amount of diffusion the current undergoes in passing through the water from the electrode to the body; and this will depend partly on the size of the electrode, partly on distance, and partly on the conductivity of the water. In other words, we have not only to consider the size and position of the electrodes electrizing the water, but we have to look upon the whole extent of water in contact with the body, as a huge electrode carrying a widely diffused current with a density diminished in proportion to its diffusion.

The device of placing a partition or diaphragm across the bath through which the immersed body passes seems well worthy of a trial. It must in an important degree influence current diffusion, and if further investigation and experience prove its usefulness it ought to become a constant feature of the "standard" electric water bath.

(To be continued.)

[6] Hydro-electric Methods.

American Climatological Association.

Twelfth Annual Meeting held at Hot Springs, Va., June 13 and 14.

The PRESIDENT, DR. S. E. SOLLY, of Colorado, made a brief introductory address, congratulating the society upon the good attendance and the choice of such a pleasant place for meeting.

DR. GUY HINSDALE, of Philadelphia, read a paper on "Recent Measures for the Prevention and Treatment of Tuberculosis," reviewing the provision made in England, Germany, France and the United States for the care and treatment of the consumptive poor.

DR. FREDERICK I. KNIGHT, of Boston, reported that the committee having this matter in hand in Massachusetts had recently secured an appropriation of $150,000 from the Legislature for the erection and maintenance of a hospital.

DR. H. L. TAYLOR, of St. Paul, under the title "Clinical Results of the Use of Tuberculin," gave a report of seventy-two cases, with a complete analysis of thirty-nine cases, treated by this method. Eighteen were third stage cases; of these seven died; five became worse; five improved, and one improved greatly. Taking the first and second stage cases together—the only cases in which the physician would encourage a patient—there was a total of twenty-one cases with a mortality of 10 per cent. In the early cases by themselves, 84 per cent. improved greatly. While not considering tuberculin a certain remedy, Dr. Taylor held that its careful use is not dangerous, and that, in beginning cases, its use should be encouraged.

In the absence of DR. V. Y. BOWDITCH, chairman, Dr. E. O. Otis, Boston, presented the Report of the Committee on Health Resorts in the United States. The work of this committee will appear from year to year in the transactions of the society and will ultimately be published in book form.

"Sunshine Statistics" was the subject of a paper by PROF. MARK W. HARRINGTON, Chief of the Weather Bureau, Washington. The sunshine—that is, the presence of a direct ray of the sun, not shut off by cloudiness—forms one of the most interesting elements of meteorology, from a sanitary standpoint. The data considered were all from the United States, with the exception of two stations in Canada. The number of years during which these data have been collected in the United States is smaller than in Great Britain, and there is a large literature and a long series of observations in English and other European publications. Professor Harrington's paper was divided into three parts: a description of the photographic sunshine recorder, with the results obtained from it; a discussion of the thermometric sunshine recorder; a comparison of the results obtained in the United States from these two records.

DR. W. F. R. PHILLIPS, of the Weather Bureau, read a paper on "Mean Atmospheric Temperatures." Throughout the month of July the average daily temperatures exceed 70° in the States bordering on the South Atlantic and Gulf coasts and in the greater part of the Middle Atlantic States.

The paper gave statistics as to the mean minimum temperatures and the mean maximum temperatures in July.

The variability of temperature or average difference from day to day is less in July and August than in any other months of the year, and it is so nearly alike in either that it is not practicable to discriminate between them in this respect. In other words, July and August are the most equable months of the twelve as regards temperature. On the Pacific and Gulf coasts the variability is little more than 1 degree. As we go northward and inland the variability gradually increases. In the latitude of Savannah it is equal to 2 degrees, and in that of Washington City 3 degrees. The greatest variability is observed in Montana and in the Dakotas, where it reaches 4 to 5 degrees.

East of the Rocky Mountains the highest one day mean temperatures have been observed generally in the central valleys and in the South Atlantic States.

The lowest mean temperatures for one day have ranged from 75 degrees on the Gulf to 55 degrees in the northern portions of the United States. In the greater part of the country the lowest mean temperatures range between 60 and 65 degrees.

DR. RICHARD C. NEWTON, of Montclair, N. J., read a paper on "Some Observations which appear to establish the Aerial Transportation of Malarial Germs." After reviewing the evidence brought forward to prove the transmission of malaria by water, he took up the literature bearing upon the belief in aerial transmission. He believes it fair to assume that in places ordinarily non-malarial the drinking water is probably the more frequent cause of ague, when it does appear, than the atmosphere; but that in places like the Campagna of Rome, the Orinoco River in South America and the jungles of East India, where malaria is so constant and so deadly, the atmosphere is the usual method of its transportation.

"The Geographical Distribution of the Mineral Springs of the United States," was read by DR. A. C. PEALE, Washington. Dr. Peale has recently compiled two maps; one showing the distribution of the 330 springs, the waters of which are put upon the market to the amount of 20,000,000 gallons annually, with a valuation of from four to five million dollars. Upon the other was plotted nearly seven hundred mineral spring resorts. The majority of the springs used commercially are in the Eastern United States and the Mississippi Valley; not because the number of springs is much greater in these regions, but because the resources are better utilized. The writer gave an interesting discussion of the subject, including a description of the springs of the various districts and showing their dependence upon the geologic structure of the country.

DR. S. E. SOLLY, of Colorado Springs, read a paper upon "The Influence of Heredity upon the Progress of Phthisis," to be published in full in this JOURNAL.

At this session, papers were also read as follows: "Etiology and Treatment of certain kinds of Cough," Dr. Beverly Robinson, New York; "A Case of Simple Chronic Pleural Effusion; its Final Outcome," Dr. J. C. Mulhall, St. Louis; "Two factors in the Production of Disease in New York City," Dr. Thomas Darlington Jr., New York.

Officers for the ensuing year were elected: President, Dr. James B. Walker, Philadelphia; Vice-President, Dr. C. E. Quimby, New York; Treasurer, Dr. James E. Hart, Colorado; Secretary, Dr. Guy Hinsdale, Philadelphia.

DR. FREDERICK I. KNIGHT, of Boston, was elected Representative to the Executive Committee of the Congress of American Physicians and Surgeons, and Dr. R. G. Curtin, of Philadelphia, alternate.

On motion of DR. F. I. KNIGHT, the following resolution was unanimously adopted:

WHEREAS, Since tuberculosis has been demonstrated to be a communicable disease, it has become doubly desirable that hospitals for the reception of the poor afflicted with this disease should be established.

Resolved, That the American Climatological Association recommend the establishment of such hospitals in every State, not only for the great suffering attending this disease among the poor, but also as a protection to the community against its spread.

At the request of the Association, DR. C. E. QUIMBY presented the following resolution:

Gentlemen of the American Climatological Association:

Duty and established custom makes it fitting that in formal manner your committee report to this Association the death upon Jan. 23, 1895, of our former President and member, Alfred L. Loomis, M.D., LL.D.

Love and respect make it imperative that in so reporting we dwell for a moment upon the many ties which united us, and make some record by which his many and varied gifts and qualities which have enriched us may be made a powerful influence upon those who are to continue this Association.

Dr. Loomis was born in Bennington, Vt., in 1831. His early education was largely under private tutors at Woodstock, Vt., while his academic degree was taken at Union College. After graduation in 1852, at the College of Physicians and Surgeons, he served the usual term in Bellevue Hospital. Very soon after starting in private practice he was appointed visiting physician to the same hospital, where he continued in active duty until the day of his final illness. For more than thirty years Dr. Loomis held the chair of pathology and practice in the Medical Department of the City of New York, and to his energy and wise efforts, more than to any other force, may be attributed the growth and prosperity of that institution.

In his professional life, Dr. Loomis was preëminently an active, energetic, sagacious physician. In all his relations to medicine and medical progress he was conservatively but uniformly progressive; never assuming an advanced position until fully or even superfluously convinced of its accuracy, he was unfaltering in its defense. A similar faith in the eventual success of truth, made him unable even to consider the possibility of failure in any purpose once undertaken.

Dr. Loomis was one of the faithful band who originated this Association, and we all know how faithfully he worked for its advancement and success. He was its first President and was again its President during the first Congress of American Physicians and Surgeons in Washington assembled and, as our delegate, was elected the President of the third Congress.

At one time efforts to merge the American Climatological Association into another national organization threatened to destroy it. Dr. Loomis averted the danger if any such existed. He had a most enviable reputation; not only in his professional but also in his private life. He was a frequent contributor to the proceedings of this Association.

In expressing our sense of personal as well as united loss in the death of Dr. Loomis, we realize that we can do him no greater honor than to present this record of his work for a perpetual stimulus to members of this Association.

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MEMBERSHIP IN THE AMERICAN MEDICAL ASSOCIATION.

This is obtainable, at any time, by a member of any State or local Medical Society which is entitled to send delegates to the Association. All that is necessary is for the applicant to write to the Treasurer of the Association, Dr. Henry P. Newman, Venetian Building, Chicago, Ill., sending him a certificate or statement that the applicant is in good standing in his own Society, signed by the President and Secretary of said Society. Attendance as a delegate at an annual meeting of the Association is not necessary to obtain membership.

On receipt of the subscription the weekly JOURNAL of the Association will be forwarded regularly.

Gentlemen already members of the Association should send their annual subscription to the Treasurer, or direct to the JOURNAL office.

All communications and manuscript of whatever character, intended for publication in the JOURNAL, should be addressed to the *Editor*, and all communications relative to the business of the JOURNAL, proof sheets returned, or in regard to subscriptions, should be addressed to THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, 86 Fifth Avenue, Chicago.

SATURDAY, JULY 20, 1895.

DEGENERACY.

This term has suddenly come into prominence, as describing a class of popular modern literature. DR. NORDAU'S work on "Degeneracy" has created a profound impression and roused bitter criticism and extravagant praise. He attempts to show that the literature and many of the popular authors of to-day are degenerate and diseased. DR. NORDAU is not only a physician and practical student of mental disease, but a clear, vigorous thinker and writer and has made out a very strong case. While a large number of physicians are not very familiar with many of the authors described, they will recognize the evidence presented, and will follow the author with keen pleasure in his grouping of symptoms and the deductions which follow. There can be doubt from the writings alone, that a large class of books widely read are the products of thoroughly diseased minds. Stories, essays, poetry and discussions of popular topics have certain stigmata of diseased and disordered reasoning that are unmistakable. The reader is referred to DR. NORDAU'S work for a full discussion of these topics.

The question is often asked by thoughtful men, why medical literature should be so vague and confusing? Why the ordinary journal articles from the *British Medical Journal* and London *Lancet*, down to the latest-born periodical which boasts of coming to "fill a long-felt want," should have so many signs of egotism, mysticism and dogmatism? Why so many writers and teachers should manifest such hysterical confidence in every new theory and alleged discovery in medicine? Why should the practice of medicine develop in certain persons intense selfishness, credulity and ego manias? These and many similar questions have often recurred to close observers, and are unexplained except on NORDAU'S theory of degeneracy. The rush of half-trained, incompetent young men to obtain entrance into the profession of medicine; and the selfish, grasping teachers and colleges who encourage this; and the isms and pathies, mystical, confused and narrow that gather up these men, are symptoms of degeneration. There is arrested development, imperfect growth, degeneration both physical and mental, with vicious training at the bottom. They come into the profession prepared to foster all sorts of errors and new beliefs, to follow blindly the teaching of some prominent medical man; gathering round the flaring lights of some novel theories, and accepting them without question. Or they may mount some pedestal of their own fancies and go down through life supporting wild theories and defending strange, unhealthy views of science, superstitious egotists, impressionists, medical mystics. Clearly they have passed over into the army of degenerates, and leave unhealthy influences and memories behind them.

All these men lack soundness of mental development; they are inharmonious in conduct and judgment; they seldom advance the cause of truth, and rarely ever do any scientific work that is sound and true. In medicine they are pessimists and critics, pulling down every good work, and pointing out bad motives in others who are working for truth. In medical colleges they are bitter rivals for places and supposed honors, and boating manias; mechanical teachers, and call themselves practical, as an explanation for want of technical knowledge. As journalists, they are chiefly noted for frantic efforts to become popular with their readers and avoid offense on every topic. The reader readily recognizes the vast number of books, papers and journals on medicine that are as dreary and destitute of scientific life as the alkalin plains of the West. The authors are simply writing out their own degeneration, pointing out evidence of dementia, melancholia, deliriums, symbolisms, mysticisms, decadents, and general failure of healthy vigorous thought.

NORDAU'S degenerates are not all writers of fiction and teachers of philosophy. They are found in medicine, in law and theology. Men who oppose reforms in preventive medicines; men who oppose laws for the regulation of practice; who want all barriers thrown down and a free open field for all kinds of practice; who would like to follow medicine as a trade and use the papers to advance their interests, the same as a circus, are evidently away over the border lines of health. Physicians who are looking for truth in the mysteries of quackery and concealment; who expect figs of thistles and grapes from thorns; who expect facts by intuition and

without labor, and become enthusiastic defenders of theories that are untested and unproven, are clearly of NORDAU's class. Medicine, like other sciences, is not a great camp-ground for parade and show and trade. It is a road narrow and rough, leading out into the unknown; a road for discovery, for investigation, lined with facts and new truths. No man of health properly trained can traverse this road without observing new facts and new laws governing them, and be impressed with the vastness of truth and his limited capacity to understand it. Modesty, conservatism and persistent study of the constantly changing evidence, are signs of health. Dogmatism, assertiveness, credulity and hysterical enthusiasm in defense of facts show an involved brain. The mania to write, to teach, to lead as authority, with no bases but the inner consciousness and some acquaintance with cotemporary medicine, and a morbid faith in the writings of foreign authors, is a sad sign of decadents.

What we need is a larger, clearer, more vigorous literature; a literature of facts, urged by robust men, who stand on strong ground and who write and teach for the purpose of stimulating further study and inquiry along the same lines.

THE EARLY DIAGNOSIS OF DIPHTHERIA AND THE PHYSICIAN'S DUTY WHEN CONFRONTED WITH CASES OF DOUBTFUL CHARACTER, PRESENTING SYMPTOMS OF DIPHTHERIA.

The existence of the Klebs-Löffler bacilli in the secretions or exudations, upon an acutely inflamed pharyngeal mucous membrane, or upon the tonsils, as demonstrated by microscopic examination of cultures, is relied upon at the present day as the prime indication that diphtheria, or, to be more specific, diphtheritic angina is the correct diagnosis in the case. Similarly, when the colonies of bacilli are located beneath the epiglottis, we recognize the presence of a diphtheritic laryngitis, which may or may not be preceded, attended, or followed by angina, but is still truly diphtheria. So firmly is the bacteriologic diagnostic test relied upon, that communities and boards of health all over the world are establishing expensive laboratories mainly for the purpose of making these culture-tests, the finding of the Klebs-Löffler bacillus in doubtful cases being held to be conclusive evidence of the presence of diphtheria. Since the diagnosis of diphtheria often involves forced isolation and so-called household quarantine, with all the vexation, inconvenience and disturbance of social and business relations resulting therefrom, it becomes a matter of considerable moment, at least to the city physician, that he shall avail himself, in all cases of supposed diphtheria, of the assistance offered by the municipal laboratory. This is the more important, because as it appears to us, in thus coming to the aid of the physician, the authorities formally relieve him of the risk and responsibility of making the positive diagnosis of diphtheria.

It must be borne in mind that under the regulations prevailing in our principal cities, the diagnosis of diphtheria falls upon a family like a veritable calamity. The Health Officer enjoys the unenviable distinction of adding new terrors to death. His advent in the family circle is regarded with undeserved but undisguised aversion, and his departure is the occasion for general rejoicing and libations of disinfectant solutions. Under these circumstances it is only natural for a physician to proceed with due caution when confronted with a case of suspicious-looking sore throat; if it be diphtheria and he should neglect to report it, he may find himself in the disagreeable position of becoming the object of official censure, in addition to being treated as a criminal and being forced to pay a heavy fine; or he may report it and incur unreasonable enmity, the consequences of which he can not foresee. Should he be so unfortunate as to make a mistake in diagnosis, or should such a mistake be only asserted by the patient, the results may be sufficiently serious, as in a case occurring in Philadelphia recently, where damages to the amount of $50,000 were claimed for an alleged error in diagnosis, the patient having been taken to the municipal hospital on the physician's diagnosis of diphtheria. In such an emergency, the possession of an official certificate from the public bacteriologic laboratory, stating that the characteristic diphtheria bacilli were present in the secretions, would have a very soothing influence upon the mind of the doctor defendant.

The duty of the physician, when called to attend a suspected case of diphtheria, is stated in a recent monograph by L. FURST[1] in terms which may be regarded as expressing the most recent views held in Europe, with reference to the early diagnosis and treatment of this important disease. In diagnosis, he gives the first place to the clinical complexus and states that the symptoms alone will enable us to make a diagnosis between diphtheritic anginas and the non-infectious forms with approximate certainty and sufficient promptness; but the bacterial diagnosis forms a valuable supplement, which should not be neglected. In the great majority of cases of supposed infective anginas, the positive proof of the presence of the diphtheria bacillus turns the scale in favor of the diagnosis of diphtheria. Where the bacteriologic tests yield no evidence of the presence of the characteristic bacillus, but only of pseudo-diphtheritic forms, streptococci, etc., the disease, according to FURST, is still to be regarded as diphtheria and is to be treated accordingly, provided the clinical symptoms call for such treatment. On the other hand, even though the diphtheria bacilli be

[1] Berliner Klinik. Sammlung Klinische Vorträge, Marz, 1895.

found, the diagnosis is not to be regarded as conclusive, unless the usual local appearances and general symptoms warrant it. As physicians can not be expected to provide themselves with bacteriologic laboratories at their own expense, the duty of making the clinical diagnosis is imperative; but it is deemed also necessary that public institutions of this kind shall be established by the health authorities in all large centers of population, in order to positively determine the character of the infection in doubtful cases of diphtheria.

As regards treatment, serum-therapy now has the floor. The duty of the attending physician is stated—rather dogmatically it may be thought in view of the unsettled questions still remaining to be solved with reference to the so-called antitoxin—as follows: the physician, in cases of suspected character, or supposed diphtheritic angina, should at once proceed to apply local treatment and administer serum injections, while adopting prophylactic measures, isolation, etc., without waiting for the report of the bacteriologic examination. Should the examination yield a negative result, the injections possibly being unnecessary, FURST regards this as only an insignificant evil in comparison with the very decided risk which had been set aside by the early treatment. He strongly urges that in all convalescing cases of diphtheria, attendants, children of the family, school companions and others, in short, wherever there is a possibility of the spreading of the bacillus diphtheriticus, that the throats should be subjected to the same bacteriologic examination, even when they present no symptoms of disease. All of those who have been exposed to infection should, without exception, be immunized.

This may be regarded as a summary presentation of the most advanced treatment of to-day. That it will be still regarded as sound doctrine ten, or even five years from now, very few will be bold enough to affirm. The argument from analogy and experience with other new remedies, would rather favor the view that the serum-therapy of diphtheria is still destined to undergo important modifications before it can be regarded as permanently established; unless, indeed, our health authorities shall take the unprecedented step of imposing, by legal enactment, this special method of treatment upon medical practitioners of all kinds, including the several restricted schools of practice—a step which they are not likely to take, or, at least, not very soon.

This subject has been under discussion at various society meetings during the past year, and it was a special feature of the session of the Association of American Physicians at Washington. DR. W. H. WELCH very accurately summed up the situation in his statement that: "The decision as to the efficiency of the diphtheria antitoxin is in the hands of the clinician and not of the pathologist." The clinician, however, was ably represented by DR. JACOBI, who declared himself to be a practical man, desirous of saving his patients, and advised that the local and systemic measures which experience has proved to be useful should not be laid aside while also resorting to serum-therapy, in cases where it seems likely that it may be beneficial. This advice is sound and the course of proceeding prudent, following both the letter and the spirit of the injunction to "prove all things and hold fast to that which is good."

THE ASSOCIATION AND ITS JOURNAL ABROAD.

In his appreciative, genial and always interesting gossip of the month MALCOLM MORRIS, editor of *The Practitioner*, takes occasion in the current mensual to say some very pleasant things about the AMERICAN MEDICAL ASSOCIATION and its JOURNAL, which it will, no doubt, gratify the members of the one and the readers of the other to see reproduced.

While it is pointed out that our ASSOCIATION is a much smaller, and less wealthy body than the British organization, MR. MORRIS, basing his judgment on the reports of the recent Baltimore meeting, holds that the scientific work which it does is no whit inferior in quality and but little inferior in quantity to that of its cis-Atlantic homologue. "The general level of the communications was high, but I was particularly struck by the importance of those dealing with surgical and gynecologic subjects." Particularizing as to these he cites the Address on Surgery by DR. C. A. WHEATON of Minnesota, who "raised a voice of warning against what the late PROFESSOR VERNEUIL called the *furor secandi*, which has sometimes given the enemies of medical science cause to blaspheme and which is looked upon with disfavor by the more conservative spirits among hospital surgeons. DR. WHEATON said that a retrospective examination of surgical literature since the introduction of anesthesia 'does much to make the thinking student wonder whether the longevity of the human family has been conserved or its physical welfare materially enhanced by modern surgery.' Has not, he asked, our feeling of safety engendered by our acknowledged perfection in surgical detail opened a wide field of surgical speculation and experiment that serves rather to enrich the surgeon (the orator refers, I presume, to intellectual enrichment) than to minimize disease and prolong life? DR. WHEATON, however, spoke with enthusiastic appreciation of the achievements of a score of brilliant men, whose work had been instrumental in placing surgery in its present advanced position."

MR. MORRIS does not agree with the orator as to the decline of the "family physician"—at least in England, where he says there is no such decline. "Some years ago it was predicted that the general

physician would in time be forced out of existence by the inevitable development of specialism; but I find it simply impossible to conceive of an evolution of the medical profession which would cause the family doctor to disappear. He is the foundation on which the whole superstructure rests. Perhaps, however, by the words 'family physician' DR. WHEATON means the general physician as distinct from the specialist," which is, of course, precisely what DR. WHEATON did mean.

In the Section on Diseases of Children, MR. MORRIS thinks one of the most interesting papers was that read by DR. LOUIS FISCHER, of New York, on "Antitoxin in Diphtheria." "He has probably tried the remedy more extensively than any other physician in the United States, and he said that while, before the introduction of antitoxin, his mortality, as nearly as he could estimate, was about 55 per cent., the proportion of deaths in over two hundred cases reported by him in April last, in which the antitoxin had been used, was about 15 per cent. He declared he would not treat diphtheria without the antitoxin any more than he would treat intermittent fever without quinin or syphilis without mercury."

We do not look aside to see whose heads the following charge may strike: "Too many American medical journals are, on the face of them, mere advertising media, evidently conducted on the principle that all is grist that comes to the mill." It is of more interest to us to note that MR. MORRIS has learned that the "AMERICAN MEDICAL ASSOCIATION is taking the lead in a movement for reform in this particular, and has shown an excellent example as regards the management of its own journal, as may be gathered from the following passage in the report of the Trustees"—the passage being that relating to the character of advertisements which is quoted in full, and the fact noted that "the report was unanimously adopted." Can it be that MR. MORRIS had in mind any other association and its journal which might participate in "a movement for reform in this particular," even if it would now have to follow an American 'lead?'"

MAY A PHYSICIAN PREPARE HIS OWN DEATH CERTIFICATE?

The above question is propounded and answered by the editor of the *Medical Press and Circular*, June 12, although the phraseology of the question is somewhat different. His question reads: "Can a Medical Man sign his own Certificate of Death?" The following is his answer:

"This question seems a somewhat paradoxical one, but everything depends upon when this particular duty is discharged. The impossibility of the act could not be disputed should the medical man have joined the great majority; on the other hand, while life remains, it would not be a difficult matter for him to fill in and sign the familiar document and record the means by which he expected to die. A case of this description is said to have actually occurred a short time ago. In an Italian town the death took place of the communal medical officer, one of whose duties consisted in visiting the dead in the public institutions, and in writing death certificates. Among his papers was found his own death certificate, with the disease from which he suffered duly recorded, the certificate being complete in all particulars save the insertion of the date of his death. When, therefore, the new medical officer appeared on the scene, all that was deemed to be necessary was to fill in the date, and the certificate was then regarded as valid. This, it must be conceded, is something quite novel in the matter of death certificates. Perhaps objection might be taken to it in a legal sense were the practice to be adopted in this country. Nevertheless, in the case of a medical man suffering from an incurable malady which was destined to destroy his life, it could only be said that he was anticipating events were he to fill up and sign his own death certificate some time before his actual decease took place, leaving some one else to add the precise date of death."

This case of the Italian physician is one where the medical certifying officer lived in an isolated locality. His certificate was, in point of fact, one of existing disease before death, not a certificate of death; and this certificate, if not countersigned by the new communal certifiant, would not be valid for any official purposes whatever. The *causa mortis* was presumably well understood by the dying physician; the fact and date of death became well known to the new-coming officer. These two or three data being brought together, a valid certificate is the result.

THE NEW YORK QUARANTINE.

Notwithstanding the fact that yellow fever has been unusually prevalent on the islands of the Spanish Main, necessitating stricter precautions at our ports of entry, the *Saratoga*, of the Ward Line Steamship Company, arrived in New York July 8 from Havana after a fast passage of a little more than three days.

No fact is clearer than that the extreme limit of the period of incubation of yellow fever is only reached at the end of the fifth day.

Some of the passengers of the *Saratoga* had provided themselves with certificates from DR. BURGESS at Havana, showing that they had been "acclimatized," that is to say, that they had had yellow fever, and therefore were not likely to transmit the disease, or to acquire it after landing. Others, however, were not so fortunate, and the Health Officer, DR. DOTY, very properly directed that those not provided with proper certificates, coming from an infected port, must either be removed to Hoffman's Island detention station, or remain in quarantine on board the ship. The steamship company preferred the latter alternative, and although there has been much growling, sanitarians generally will sustain DR. DOTY in his course.

THE AMERICAN MEDICAL ASSOCIATION PRESS.

It is with some satisfaction that we point to the illustration in colors accompanying Dr. Sterne's article on "Toxicity as an Etiology of Nervous Diseases." This was printed on our own presses and is a handsome specimen of press work. The ASSOCIATION PRESS imprint will in years to come, we trust, stand for all that is excellent in mechanical work, and good in itself as well.

CORRESPONDENCE.

A Post-mortem Examination in a Case of Appendiceal Abscess.

TECUMSEH, MICH., July 13, 1895.

To the Editor:—Mrs. B., age about 54 years, was confined to her bed on the first of July of the present year. The family sent for a homeopath, who appears to have had not the slightest idea of the nature of her illness, but apparently satisfied the gullibility of the friends until the death of the patient, which occurred ten days from the commencement of the attack. After death, the friends became anxious to know the cause, and whether, or not, the patient died from neuralgia of the heart, which was the cause assigned by this ignorant homeopath. At the solicitation of the family, three physicians (including the writer), and two homeopaths attended the post-mortem examination. Upon opening the abdominal cavity, there was found a ruptured appendiceal abscess which occurred a few hours before death; doubtless the rupture was produced by the attendant pressing upward upon the cervix of the uterus, in such a manner that the fundus came in contact with the abscess, in an effort made by him a few hours before the death of the patient to reduce an imaginary displacement.

The point which the writer desires to make in bringing this case before the profession is the necessity of making an early diagnosis in appendicitis, and the absolute necessity of calling in a competent surgeon, upon whom shall fall the responsibility of operating or not operating.

Very respectfully, J. F. JENKINS, M.D.

Treatment of Hernia Cerebri.

CORONADO, CAL., July 6, 1895.

To the Editor:—Moved by a very interesting and instructive paper, recently published in the JOURNAL, on brain surgery, in which Volkmann's treatment of fungus cerebri by the curette is mentioned, I wish to report a case successfully treated, in my first year of practice, by a different process.

In July, 1838, my father (University of Pennsylvania, 1809) was called several miles in the country to a child about 3 years old, who had been kicked by a horse, and I went with him. We found a tumor over the left parietal, the child in deep stupor, and other indications of compression. The knife was given to me and I made a crucial incision, brain and clots of arterial blood following the knife. The dura was much torn and was trimmed after eleven pieces of bone were removed. Fearing erysipelas few sutures were used and water dressings were applied.

The case was left in my charge, and to our surprise and gratification the recovery was complete.

A few weeks after I was called, and found a fungus cerebri, half the size of an egg, cut horizontally in two. I proposed to excise it, but the mother objecting, I took an old tea caddy, hammered it so as to make a convex compress, and, after first covering the fungus with lint smeared with cerate, applied my lead compress, and over that a bandage. This was removed and reapplied every day, and the result was complete absorption, and a most satisfactory cure, without any impairment of the intellect.

F. W. TODD, M.D.

To Prevent Accidents.

CHICAGO, July 12, 1895.

To the Editor:—I noticed that in several of the eastern cities it was the custom of the street car companies to have cable and electric cars stop before crossing the street. It seems to me that this would be much safer for pedestrians and drivers than the plan followed here, and it would add much to the safety of bicycle riders.

Yours truly, I.

American Medical College Association.

RIDGEWAY, S. C., July 12, 1895.

To the Editor:—I want the list of names of all medical colleges belonging to the American Medical College Association. Where can I get it and how much will it cost?

Respectfully, S. S. L.

ANSWER:—Apply to Dr. Bayard Holmes, Secretary, 104 E. 40th Street, Chicago, Ill.

Dr. Jas. E. Reeves.

CHICAGO, July 14, 1895.

To the Editor:—Why don't you publish the address of Dr. James E. Reeves? I would be glad to send 50 cents for a copy of his report, if I knew his address. D.

ANSWER:—The address is Chattanooga, Tenn.

BOOK NOTICES.

A Treatise on the Nervous Diseases of Children, for Physicians and Students. By B. SACHS, M.D. Cl., 8vo., pp. 666. New York: William Wood & Company. 1895.

American medical literature will soon rival the French in the extent and variety of its monographs, and works devoted to specialty subdivisions. We should also add that the quality of American medical literature is constantly improving. The book under consideration is well written and exhaustive. It contains eleven chapters devoted to General Nervous Diseases; twelve to Organic Diseases of the Nervous System; eight chapters to Diseases of the Brain, an introduction and an appendix; a copious index concludes the work. There are 162 illustrations. The author's conservatism is well shown in the chapter on chorea, wherein he rejects the large doses of arsenic recommended by some, and takes issue with Erb regarding the use of electricity. In regard to the question of school-going the author says: "A special caution is necessary as regards the question of attendance at school. Every choreic child, however mild its attack may be, should be kept from school, both for its own sake and for the sake of the other pupils who might imitate the disease. I have sometimes allowed myself to be persuaded to permit a child with a mild form of chorea to continue at school; in almost every instance I have had reason to regret it, for nothing is better calculated to bring out severe chorea than the competitive spirit that obtains in most schools. Periods of examination are fraught with greatest danger to those children who have had attacks in early life. The atmosphere of the school-room seems to have a depressing influence upon such children, and among the wealthier classes far better progress can be made in the ordinary studies if the child is instructed at home than if it is taught at school. It is necessary for the physician to take a firm stand on this question, or else his treatment of the case will be thoroughly unsatisfactory."

The book is a valuable one, and worthy of praise.

Remote Consequences of Injuries of the Nerves and Their Treatment. An examination of the present condition of wounds received in 1863-65, with additional illustrative cases. By JOHN K. MITCHELL, M.D., Assistant Physician to the Orthopedic Hospital and Infirmary for Nervous Diseases, Philadelphia; Lecturer on Physical Diagnosis in the University of Pennsylvania. In one handsome 12mo volume of 233 pages, with 12 illustrations. Cloth, $1.75. Philadelphia: Lea Bros. & Co.

More than thirty years have elapsed since Acting Assistant Surgeons S. Weir Mitchell, George R. Morehouse and William W. Keen made their special investigations on gunshot and other injuries of the nerves, in the temporary U. S. Army Hospital in Christian Street, Philadelphia. The report which these gentlemen prepared in 1864 may be read now with all the interest which attaches to the recital of a traveler upon a new and hitherto unknown territory. "Never before in medical history has there been collected for study and treatment so remarkable a series of nerve injuries," say the authors. The opportunity was afforded by the setting apart of a hospital for diseases and injuries of the nerves, by order of Surgeon-General Hammond, and the report cited is now a medical classic.

The book now under consideration, by the son of the senior medical officer mentioned, may be said to be the sequel to that report, since it is a pretty successful attempt to trace the history of many of the cases treated in the Christian Street and Turner's Lane Hospitals in 1863.

The military surgeon and the neurologist have alike an interest in this class of cases, and we predict that the little book will meet with favorable reception. The records of the Pension Office, which should long ago have been published as a whole, under proper editorial supervision, are here made use of with reference to this class of cases, with judgment and skill.

Indigestion. An Introduction of the Study of Diseases of the Stomach. By GEORGE HERSCHELL, M.D. Lond. Second Edition 8vo., cl., pp. 343. New York: G. P. Putnam's Sons. London: Bailliére, Tindall & Cox. 1895.

This book "does not profess to be a complete treatise upon all the disorders of the digestive apparatus, nor to exhaust the subject of diseases of the stomach." It is intended as an elementary manual for students and as such it very fairly meets the indications.

The general nutrition of the body depending, as it does, upon the function of stomach and intestinal digestion, it follows that the pathologic processes of all tissues, are profoundly affected by the manner in which that function is performed. The study of digestion, and its derangements or disturbances, must therefore be of interest to specialists as well as general practitioners desiring to bring their knowledge up to date. Therefore too many fundamental works of this character can not be had, and the book of Dr. Herschell is very welcome. That it has already reached a second edition is a testimonial to the intrinsic merits of the work.

The Art of Massage. Its Physiologic Effects and Therapeutic Applications. By J. H. KELLOGG, M.D., 8vo., cl., pp. 282. Battle Creek, Mich.: Modern Medicine Publishing Co. 1895.

Massage properly applied is of interest to all divisions of medicine and surgery and in some is indispensable. The author has in this volume well classified the different procedures employed by expert masseurs, and the work is well illustrated. As a text-book for nurses' training schools we think it admirably serves the purpose. Less elaborate than Graham it is still complete enough to furnish the necessary information, but perfection in massage can only be reached by a few. No amount of drill can supply delicacy with strength to persons naturally deficient. All, however, may be improved by study and practice. The style of the book is such as to adapt it to recitations and reference rather than continuous reading. We commend the book.

Transactions of the New York State Medical Association for the year 1894. Volume XI. Edited for the Association by E. D. FERGUSON, M.D. 8vo., cl., pp. 751.

This large and handsomely printed volume contains the proceedings and papers read at the meeting of the Association held at New York City Oct. 9 to 11, 1894, under the Presidency of Dr. Charles D. Strong, an account of which was printed in this JOURNAL in due course.

Memorial notices are inserted of Drs. J. B. Andrews, M. K. Hogan, William Goran, N. M. Perry, W. T. White and John Young.

Reports of the District Branches, and the annual report of the Council are included in the volume, which is printed in New Hampshire. The widely distributed lecture of Dr. H. O. Marcy on Hernia, adorns the volume, with its usual grace and perspicuity.

The Association is flourishing, having upwards of 800 Fellows on the active list, and the papers in this volume are many of them of such merit as to permanently enrich medical literature.

Transactions of the Medical Association of Central New York. Buffalo: Buffalo Medical and Surgical Journal print. 1895.

This brochure contains the papers and proceedings of the twenty-seventh annual meeting of the Medical Association of Central New York, held in Buffalo, Oct. 16, 1894, under the Presidency of Henry L. Elsner, M.D.

The fortunate subscribers of our contemporary, the *Buffalo Medical and Surgical Journal*, have had the opportunity of reading these papers. Others will be instructed and entertained by reading the little volume, which contains sixteen excellent papers.

Transactions of the Southern Surgical and Gynecological Association. Vol. VII. Published by the Association. 1895.

This elegant volume of 336 pages comprises the proceedings and papers of the seventh session, held at Charleston, S. C., Nov. 13, 14 and 15, 1894, under the presidency of Cornelius Kollock, M.D. We printed in our issue of Dec. 15, 1894, a full abstract of the papers read at the meeting. The biography of Warren Stone by the late A. B. Miles, and the sketch of Marion Sims by Edward Souchon can not fail to be read and re-read with interest.

SOCIETY NEWS.

Third International Congress of Physiologists.—Bern, Sept 9 to 13, 1895.—At its opening session on Sept. 10, 1889, the first International Congress of Physiologists adopted the following resolutions:

1. An International Congress of Physiologists shall be held triennially, with the object of contributing to the advancement of physiology by affording physiologists of various nationalities an opportunity of personally bringing forward experiments, and of exchanging and discussing their views together, and of becoming personally acquainted one with another.

2. Membership of the Congress shall be open to all professors and teachers of biologic science, belonging to a medical faculty or any other similar scientific body, as well as to all scientific men engaged in biologic research.

3. The sessions of the Congress shall be devoted to physiologic communications and demonstrations. Further, communications relating to original research in anatomy, general pathology, and pharmacology, are acceptable in so far as they present features of general biologic interest.

4. It is desirable to keep the communications as far as possible demonstrable and experimental in character.

5. No official report of the work of the Congress shall be published.

The following regulations were discussed and adopted for conducting business at the sessions of the Congress:

1. The languages recognized as official at the Congress are English, French and German.

2. At each sitting, two presidents for the next sitting are chosen by the meeting, on the proposal of the chairman.

3. At the opening of the Congress, the meeting elects for each of the official languages, a general secretary, who shall superintend the preparation of the minutes of the meetings.

4. The minutes are written in the three official languages by three Secretaries chosen at each sitting, by the President in the chair. Each person who makes a communication shall sign the protocol of his own communication. The President in the chair shall confirm the correctness of the minutes for the whole sitting.

5. The length of a communication may not exceed fifteen minutes. When that period has been exceeded the President must ask the meeting whether it desires the communication to continue farther.

6. A motion backed by three members for the closure of a communication or of a discussion, must be immediately put to the vote.

7. The press shall not be officially admitted to the Congress; each member is free to send private communications to scientific journals.

On Sept. 12, 1889, the first International Congress of Physiologists passed the following resolutions:

1. The signal success of the Congress shows the appropriateness of its inception and the desirability of continuing its meetings.

2. The second International Congress of Physiologists shall be held in the year 1892 in Belgium or in Switzerland, and, if possible, in a town in which the community employs the French language.

Accordingly in August, 1892, the second International Congress of Physiologists was held at the Liége in the Laboratory of Professor Fredericq, and, as before, official meetings were held for three successive days, and were devoted chiefly to demonstrations and experiments.

The second Congress, which met at Liége, resolved on the recommendation of its Executive Committee that the third International Congress of Physiologists be held in September, 1895, from the 10th to the 14th of the month, at Bern, Switzerland. Professor Kronecker, Director of the Physiologic Institute of the University of Bern, has kindly expressed his readiness to afford to members of the Congress all facilities for demonstration and experiment, as well as for the exhibition of scientific apparatus. It is particularly desirable that those intending to avail themselves of Professor Kronecker's assistance should let him know what their requirements are not later than June 30.

In connection with the Congress, an exhibition of physiologic apparatus will be held. Exhibits may be contributed by all members of the Congress, by the directors of physiologic laboratories, and by makers recommended by any member of the Congress, or by the director of a physiologic laboratory. The exhibition of apparatus will open two days before the Congress, and will close two days after the Congress.

I desire hereby, on behalf of the Executive Committee, to invite you to attend the third Congress, September 9 to 13 of this year, at Bern. Should you intend to be present, kindly notify your acceptance, and, if possible, the title of your communication, either to Professor Hugo Kronecker, Bern, or to myself, before August 1.

Each member of the Congress is required to contribute the sum of 10 francs toward defraying the expenses of the meetings; he will obtain in receipt from the President, Professor Kronecker, a card of membership of the Congress.

Titles of communications may be sent to Frederic S. Lee, Secretary of the American Physiological Society, Columbia College, New York City.

I am, dear Sir, yours truly,

C. S. SHERRINGTON,

General Secretary for the English Language,

On behalf of the Executive Committee.

27, St. George's Square, S.W., London.

NECROLOGY.

E. R. PALMER, M.D. The sudden death of this eminent physician and surgeon, which occurred on the night of the 6th inst at the Norton Infirmary, Louisville, Ky., produced a profound sensation of sorrow, not only in that city, his home from early boyhood, but throughout the whole land wherever his name and reputation were known. While returning from a pleasure ride on his bicycle on the night in question, when crossing the railroad tracks at their intersection with the boulevard, the Doctor lost control of his wheel, which ran against the curbing and caused him to fall to the pavement. His head struck the curbing when he fell and the skull was fractured. He was removed in an ambulance to the Norton Infirmary, but died just after midnight, about two hours after the accident occurred, without recovering consciousness, although nearly a score of his fellow-practitioners were at his bedside, busily and anxiously engaged in using their best efforts to save the life of their friend and co-worker. Dr. Edward R. Palmer had lived in Louisville since he was 6 years old. He was born in Woodstock, Vt., Nov. 18, 1842. His father was Prof. Benjamin R. Palmer, who was a leading physician in Vermont, and moved to Louisville in 1848. He accepted the chair of surgery in the Medical Department of the University of Louisville. Dr. Palmer graduated from the University Medical College in 1864, during the war. He immediately became assistant surgeon in the U. S. Army and was assigned to hospital duty in Louisville. He also served in the same capacity at Lebanon, Ky. At the close of the war he located in his father's office in Louisville, which he was occupying at the time of his death. His father died in 1865.

Dr. Palmer was extremely successful in his practice, which at that time was that of a regular practitioner. He continued in this manner until ten years ago, when he began as a specialist in genito-urinary troubles. His career as a physician was unusually brilliant. He was called to the chair of physiology in the medical department of the University of Louisville in 1868, just before his marriage. Since then, Dr. Palmer has occupied the chair without interruption, and was noted for his splendid lectures. He was also clinical teacher of diseases of the chest and of genito-urinary diseases. In 1893 he was elected President of the Association of Genito-Urinary Surgeons, of which he was a member at the time of his death. He was also one of the promoters and members of the Surgical Society of Louisville, and a member of the Louisville Academy of Medicine, Medico-Chirurgical Society, Kentucky State Medical Society, AMERICAN MEDICAL ASSOCIATION, and a Fellow of the College of Physicians and Surgeons of Louisville.

The Doctor was a brilliant and forcible writer, and his contributions to the medical and surgical journals were always interesting and instructive. He was enthusiastic in everything he undertook. He was a prominent club man and was welcomed everywhere for his genial temperament. He had a reputation for after-dinner speeches and brilliant conversation—moreover, he was a good singer, and his rendering of such selections as "My Old Kentucky Home" has charmed many brilliant companies around the festal board.

Dr. Palmer leaves a widow and three grown children—a daughter and two sons. The latter, Edward R. Palmer Jr. and J. B. Palmer, were recently graduated from Princeton University and are now attending the medical department, University of Louisville, from which they will graduate next March.

Dr. Palmer's funeral took place from Christ Church Cathedral on the 8th inst., and was one of the largest that ever occurred in the city. The principal physicians and surgeons of the city were in attendance, a large number of whom served as active and honorary pall-bearers. Many of the professional brethren of the deceased from neighboring cities were also in attendance.

FIDELIO B. GILLETTE, M.D., formerly of Brooklyn, died July 1, at De Ruyter, N. Y., in his sixty-first year, from an attack of renal disease. He was an alumnus of the University of Pennsylvania, class of 1856. He was a resident of Brooklyn for over twenty years, and early joined the county medical society. He was a member of the Physicians' Mutual Aid Society. He was a gentleman of refinement and those generous traits of character that naturally and readily tend to popularity. Interment took place at Shiloh, N. J.

PAUL J. TAYLOR, M.D., of Hoboken, N. J., died July 6. He was taken seriously ill while at the bedside of a patient. He

became comatose soon after being taken to his home and did not rally. His death is attributed to cardiac disease. He was a graduate of the University of New York in the class of 1870. He was city physician of Hoboken from 1886 to 1887, for fourteen years examining physician of the Metropolitan Life Insurance Company, and was the medical examiner for several fraternal benefit societies. He was a member of a large number of social organizations and an influential officer in several. A widow and four children survive him. He was 58 years of age.

GERRARD GEORGE TYRRELL, M.D., of Sacramento, Cal., died June 8, 1895, aged 64. He was born at Dalkey, Dublin County, Ireland, in 1831, and was graduated from the Royal College of Surgeons, Ireland, in 1856, and from King's and Queen's College of Physicians in 1859; after graduation he came to the United States and established himself in practice in Milwaukee, Wis., where he remained until 1861, at which time he moved to Grass Valley, Nev., and in 1868 to Sacramento. He was an old and honored member of the AMERICAN MEDICAL ASSOCIATION.

PUBLIC HEALTH.

Limited School Quarantine for Infectious Diseases.—The following scale of school detention of scholars and teachers has been adopted by the Pennsylvania State Board of Health. The recommended time of quarantine, for persons who have been exposed to the infective diseases, after which those persons may safely be admitted again to schools, if they continue in good health and have taken proper measures for disinfection: for diphtheria after twelve days; smallpox eighteen; measles, eighteen; chickenpox, eighteen; mumps twenty-four; whooping-cough, twenty-one. Adults may be admitted at once, if they disinfect their clothes and persons.

Death Rates of European Cities in 1894.—Following are the death rates per 1000 of the principal European cities for the year 1894: Bristol 15.4, Frankford-on-Main 16.5, Berlin 17.2, London 17.7, Leeds 17.8, Brussels 18.1, Hamburg, 18.1, Amsterdam 18.3, Birmingham 18.5, Leipzig 18.7, Copenhagen 18.7, Turin 18.8, Zurich 18.9, Genoa, 19.0, Stockholm 19.4, Rome 19.6, Christiana 19.6, Nice 19.7, Glasgow 20.0, Paris 20.2, Rotterdam 20.2, Manchester 20.4, Lyons 20.9, Dresden 20.5, Berne 21.0, Venice 21.6, Bordeaux 21.9, Bologna 21.9, Prague 22.1, Vienna 22.8, Cologne 23.1, Munich 23.7, Liverpool 23.8, Budapesth 24.4, Dublin 24.7, Milan 25.0, Naples 27.7, Marseilles 28.3, Barcelona 29.6, Havre 29.8, Bucharest 29.9, Trieste 30.1, Rouen 31.3, St. Petersburg 31.4, Moscow 34.1.[1]

Nature's Compensations.—Vital statisticians have frequently commented upon the fact that after a period of devastating wars the birth rate runs high, that after an epidemic the death rate falls. Whatever the principle involved it is obviously operating in Milwaukee this year. One of its local journals of the 9th inst., says: "This morning there were only two cases of contagious diseases in the city and these were cases of scarlet fever. Last Saturday the weekly report gave the number of scarlet fever and diphtheria cases in the city as eight each. To-day diphtheria is wiped out, smallpox has given no signs of revival and scarlet fever dropped down to two cases. Never in ten years past has this record been equalled or even approached and the members of the Health Department feel correspondingly pleased."

Did Not Blame the Americans.—Two severe outbreaks of ptomaine poisoning have recently occurred in Great Britain—in one of which all the members of two families, numbering twenty-two persons became seriously ill after eating "tinned" meat and one died; in the other, a man purchased some veal on a Saturday and, having shared it with two other families, all partook of it on the Sunday, and the following morning nearly a dozen persons who had eaten of the veal were attacked with violent purging, vomiting, etc., and one also died. The singular feature, common to both these outbreaks, is that in none of the accounts which have come under observation has there been any charge that the sickness and death were due to the poisonous meats which, it is well known Americans are in the habit of putting up for export to the unsuspecting foreigner.

Preventing a Stench Nuisance.—The Conseil d'Hygiene of Paris has suggested to the municipality that all butchers in that city be required to prevent the nuisance arising from the putrefactive decomposition of bones, fat, meat trimmings and other *debris* accumulating about their shops and premises. At the request of the Conseil, M. Nocard has recommended a disinfectant for this purpose, which is at once cheap, easily applied, free from danger, effective and does not destroy the value of the *debris* for industrial purposes. This consists of a 2 per cent. zinc chlorid solution to be used in the following manner: the butcher will provide a large wooden vat of 120 liters (30 gals.) capacity, in which will be mixed 2 liters of zinc chlorid solution at 40 degrees, with 100 liters of water; the scraps, bones, etc., will be thrown into this vat and kept there until removed. A sufficient quantity of the 2 per cent. solution must be added from time to time to maintain the strength necessary for disinfection and exhausted by use.

Errors of Diagnosis in Infectious Diseases.—In *Public Health* for July we find a brief analysis of cases of the above nature, in the last Annual Report of the Metropolitan Asylums Board of Greater London. This monster organization has under its control accommodations for 12,000 patients, and carries an average population of not less than 9,000 a day. In this figure are included the "imbecile" as well as infectious classes. The number of admissions to the infectious disease hospitals was nearly 17,000 in 1894; of scarlet fever alone there were 11,596 admissions. These figures are quoted solely to show the enormous volume of business conducted by the Board. Concerning the question of errors in diagnosis, the analysis in *Public Health* remarks:

"In the course of 1894 no fewer than 864 patients, or a percentage on the total admissions of 5.2, were, after admission at the fever hospitals of the Metropolitan Asylums Board, found not to be suffering from the diseases mentioned in the medical certificates upon which they were removed to hospital. The largest number of cases of mistaken diagnosis admitted at any one hospital was, as in previous years, at the Eastern Hospital, where 258, or 9.8 per cent., were received. Of the patients admitted to the hospital ships, 24 were not suffering from smallpox at the time of admission. Of this number: four were mothers admitted with their infants; nine were infants admitted with their mothers; one had no symptom of disease at the time of admission, but was sent to hospital as convalescing from smallpox; and ten suffered from other diseases, a list of which is given in the report of the medical superintendent. From these figures it appears that the actual number of mistakes in diagnosis among the patients admitted to hospital was 10, or only 0.8 per cent. If, however, we consider the total number of cases certified as smallpox, and removed to the wharves and medically examined there, we find that of 1,263 cases 155, or 12.2 per cent., were in the opinion of the managers' medical officers not suffering from smallpox, and were returned to their homes, with the exception of four, who were transferred to fever hospitals. It would be interesting to hear the other side of these cases. Did any of these patients, after being returned home prove to have the diseases originally notified? Do mistakes in diagnosis occur only on the part of private medical practitioners? There is a general consensus of opinion in large towns that isolation accommodation for doubtful cases form an indispensable part of the administrative machinery of fever and smallpox hospitals; but this is almost entirely wanting in the metropolis."

[2] Rev. Int. Med. et Chir, Prat., No. 11, 1895.

Cause of Cancer.—Dr. James Braithwaite of Leeds, Eng., editor of the *Retrospect of Medicine*, does not positively assert, but he confidently believes, that he has discovered the cause of cancer and sarcoma in a spore-bearing mycelium which he has succeeded in fully demonstrating to his own satisfaction and which he illustrates in a descriptive paper.[1] So well satisfied is he of the correctness of his conclusions that he leaves to others their disproof, and proceeds to discuss the question, Is any practical result likely to accrue from this discovery? So far as he can see at present the answers may be given as follows: 1, the cure of a cancerous growth actually in existence does not seem more probable [by reason of this discovery] but less probable, because the enemy is so subtle, so penetrating, and so indestructible—proved by immersion in liquor potassæ for an hour without killing it—that it almost seems impossible to eradicate it; it is, however, better to have an enemy exposed to view than one invisible. But it seems likely that, after removal of a growth by operation, it would be better not to close the wound absolutely, but to keep the surface in contact with dressings soaked in glycerin containing liquified carbolic acid and also perchlorid of mercury—the carbolic acid partly to relieve the pain; glycerin penetrates the tissues and carries with it any substance in solution. 2, it seems not improbable that cancer of the breast in women past the child-bearing period may be altogether prevented by closing the orifices of the milk-ducts in the nipple by a small surgical operation or by the use of a fine electric cautery; for the fungus must enter the breast *via* the milk-duct orifices. 3, it is possible that men may have to return to a certain Mosaic rite in order to save wives from uterine cancer, unless the daily use of soap will do away with this necessity. 4, in order to prevent the fearful mortality from cancer in low-lying, wet-soil districts the bodies of persons dying from cancer must be buried at a great depth or they must be cremated. Moisture is absolutely necessary for the formation of zygospores, spore masses and spore-bearing mycelium. It does not seem probable that the fungus necessarily spends part of its life in the earth, but no doubt when the interment is at no great depth the earth will become infiltrated if it is constantly wet and floods will carry the spore masses all over the country. 5, a fungus can attack only effete tissues. We are, ourselves only tools of nature; we must not rust from luxury and idleness. Business men must be careful how they retire—for, just as tuberculosis is favored by excessive work and poor food, so the growth of a fungus is likely to be favored by the effete material present in the system and engendered by opposite conditions.

Prophylaxis of Consumption.—The researches of Professor Cornet into the mode of propagation and the life history of the tubercle bacillus and the methods of prophylaxis based thereon which he suggested have profoundly influenced the restriction of tuberculosis all over the world. As is so frequently the case many of his disciples, with more zeal than discretion, have carried prophylactic measures to extremes and have advocated wholly unnecessary precautions and restrictions. In a paper recently read before the Berlin Medical Society[1]—the gist of which was the results obtained in the Prussian prisons by caring for the sputum—Cornet makes it clear how comparatively simple are the means necessary to limit the spread of consumption. He pointed out that microörganisms with a moist surface could not be detached by a stream of air passing over them, and exploded the idea that the breath was the source of infection in pulmonary tuberculosis, making clear at the same time that this source must be looked for in the excretions from the patient. He proved that the bacillus showed its earliest and widest development at its point of entrance into the organism or in the corresponding lymph glands, and that in man the lungs and bronchial glands formed this point in the large majority of cases. He looked upon the sputum and especially dried sputum, as almost the sole cause of the extension of pulmonary tuberculosis, and was confirmed in the conclusion that by preventing the sputum from drying and becoming dust, an efficient prophylaxis of tuberculosis was possible. Precautionary measures in this direction have been in force for the last few years in Germany, and especially in Prussia, where the authorities have interested themselves in the question. Cornet gives statistics showing the mortality from tuberculosis in the prisons in Prussia and Bavaria. The figures in both countries show much the same proportion from 1881 to 1887. Since 1887 the rate in Prussian prisons has fallen very rapidly and in the period 1892-94 was only 81.2 per 10,000 inmates—hardly one-half the former figure. These prophylactic measures are not, however, generally adopted, and in Bavaria, the view of Bollinger—that predisposition is a potent factor in tuberculosis—has interfered considerably with their adoption. Cornet's ideas have lately made progress, and now the figures, which have remained stationary up to 1891, have begun to show a reduction. Similar results are obtained from asylums: a fall from 184 to 150—*i.e.*, about one-sixth—is seen in Prussia, while no reduction takes place in Bavaria. The statistics of the Catholic nursing orders of Prussia are very interesting. In these, the disposal of the sputum has been attended to with great care, and in consequence, during the last six years, a decided improvement is shown; while in 1887 the rate stood at 100 in 10,000, in 1893-94 it had sunk to 67. Going into larger figures, Cornet quotes those issued by the Royal Statistical Bureau of Prussia. From the year 1875, when reliable information was first obtained to 1887, the death rate from tuberculosis averaged 31 in 10,000 inhabitants. Since the latter year it has sunk from year to year to 25 in 1893. Stated generally, there were in Prussia alone between the years 1887 and 1893, 70,000 fewer deaths from tuberculosis than were expected from the average of previous years. A similar disease has been noticed in Hamburg, where precautionary measures have been instituted, and a comparison made with towns which have not adopted them shows that in the latter little or no decrease in the mortality from tuberculosis has occurred.

Health Reports.—The following health reports have been received by the Supervising Surgeon-General, Marine-Hospital Service:

SMALLPOX—UNITED STATES.

Michigan: Smallpox reported July 6 at Battle Creek, Danby Township, Detroit, Olivet and Warren.
Ohio: Cincinnati, July 5 to 12, 1 death; Cleveland, June 1 to 30, 1 case, 1 death.
Wisconsin: Milwaukee, June 1 to 30, 2 deaths.
Tennessee: Memphis, July 7 to 13, 2 cases.
Louisiana: New Orleans, June 8 to 29, 6 cases, 7 deaths.
Pennsylvania: Philadelphia, June 30 to July 6, 1 death.
Missouri: St. Louis, June 1 to 30, 2 deaths.

SMALLPOX—FOREIGN.

London: June 22 to 29, 25 cases, 1 death.
Dublin: June 22 to 29, 18 cases.
Antwerp: 15 to 22, 1 death.
Batoum: June 18 to 25, 5 cases, 3 deaths.
Birmingham: June 22 to 29, 1 case.
Calcutta: May 28 to June 1, 34 deaths.
Gibraltar: June 16 to 23, 1 case.
Hiogo: June 1 to 8, 1 case.
Hong Kong: May 25 to June 1, 1 death.
Liverpool: June 22 to 29, 1 case.
Manchester; June 22 to 29, 1 case.
Montevideo: June 1 to 8, 1 case, 1 death.
Moscow: June 15 to 22, 2 cases, 1 death.
Naples: June 15 to 22, 2 cases, 2 deaths
Odessa: June 15 to 22, 3 cases, 1 death.
Rio de Janeiro: June 8 to 15, 9 deaths.
Rotterdam: June 22 to 29, 1 case, 1 death.
Warsaw: May 26 to June 2, 2 deaths; June 8 to 15, 1 death.

CHOLERA.

Bombay: May 28 to June 4, 1 death.
Calcutta: May 25 to June 1, 45 deaths.
Hiogo: June 1 to 8, 6 cases, 5 deaths.

YELLOW FEVER.

Santiago de Cuba: June 30 to July 6, 25 deaths.
Vera Cruz: June 27 to July 4, 9 deaths.

[1] The Lancet, June 29, 1895.

[1] Berl. Klin. Wochenschrift, May 20, 1895. The Practitioner, July, 1895.

MISCELLANY.

Change of Address.—Dr. W. Xavier Sudduth, from Minneapolis, Minn., to 100 State Street, Chicago.

Was this "Writ Sarkastik?"—We find the following in the "Literary Notes" of our esteemed contemporary and namesake, the *British Medical Journal* of June 29: "*Medicine* is the name of a new monthly journal of medicine and surgery published at Detroit under the editorship of Dr. Harold N. Mozer. Our new contemporary presents a very creditable appearance, and to judge from the first three numbers it will be a valuable addition to medical journalism. We congratulate both the editor and the publisher (Geo. S. Davis, whose name is favorably known in connection with the Index Medicus), on the fact that the trail of the advertiser is not over their journal as it is over so many trans-Atlantic periodicals." The *Journal* is itself so immaculately free from the "trail of the advertiser" that our colleague will, no doubt, appreciate the "congratulations" not less highly than the change made in the orthography of his patronymic.

Origin of Syphilis.—At the beginning of the sixteenth century it was claimed on the authority of Roderigo Diaz and Oviedo that syphilis was a disease imported from America by the companions of Columbus. This theory held undisputed sway until near the close of the last century and it is still held by many physicians. It was about 1493 that the disease first came to be spoken of and it was natural to attribute it to the most marked contemporary event—the discovery of America. It would seem very astonishing, however, that it should suddenly have such a great extension over Europe if it had not existed there before. Puschmann has collected evidence which proves to him that the disease existed in Europe during the Middle Ages and even in antiquity. The disease remained unknown, however, until about the time of Columbus' discovery when it caused serious epidemics which attracted attention to it. The American origin was rendered all the more probable because the guaiac wood, then used in its treatment, came from America. It seems probable, nevertheless, that it existed before in several lands—especially among the negroes and the Moors driven from Spain.[1]

Diagnosis of Serous Effusions by Acetic Acid.—It is sometimes a very delicate task to determine in cases of serous effusion whether we have to deal with an inflammatory exudate, as in pleurisy or peritonitis, or a simple transudation, as in hydrothorax and mechanical ascites. Primavera has facilitated this research by showing that acetic acid precipitates the paraglobulin held in suspension in inflammatory exudates. Rivolta points out a practical means of using this test for differentiation. Two successive examinations are made, controlling each other. In a small quantity of fluid, attained by puncture and filtered, is placed a drop of slightly diluted acetic acid; in the case of an exudate a slight white precipitate is immediately formed, which follows the drop as it falls to the bottom; in the case of a transudate the liquid is colored throughout. 2, in a test tube are placed 200 to 300 c.c. of distilled water to which has been added one drop of glacial acetic acid to each 100 c.c. of water; a drop of the liquid to be examined is allowed to fall into the tube, when, in case of an exudate, it will form a whitish-blue flocculent precipitate which falls to the bottom. If some more drops of acetic acid be added this precipitate completely disappears. This test must always be carried out to exclude a precipitate of insoluble mucin.[2]

Disinfection of Wells.—Wells are frequently infected, especially when the country has been inundated. As they are often situated close to dung hills the drainage of the latter often becomes mixed with the well water, forming a culture medium for microbes. It will be some months before the well furnishes pure water again—that which trickles from the walls being contaminated from the deposits left on the surface of the bricks and between them. Frank has recently recommended the following procedure which he employs on the banks of the Rhine. A plate, containing 50 to 100 grains of bromin, is suspended in the mouth of the well; as is known, bromin is volatilized in air, and its vapors are heavier than the latter, hence there is formed a cloud of vapor which falls slowly into the well, skirting the surface, penetrating into crevices and completely destroying organic matters. At the bottom of the well the bromin is dissolved and, being heavier than water, the mass of liquid is completely penetrated by the disinfecting agent; the water retains a slight taste of bromin for some time, which is not very agreeable, but it is purified and made perfectly healthy.[1]

The Index Medicus.—The following paragraph, quoted from the London *Lancet*, for June 29, shows that there are in England medical workers who duly appreciate the Index Medicus. There are endowed medical organizations in that country that could legitimately and "without feeling it,"—as the saying is—contribute to the $2,000 subscription suggested by Librarian MacAlister.

"In another column will be found a letter from the Resident Librarian of the Royal Medical and Chirurgical Society concerning the imminent decease of that invaluable publication, the Index Medicus. When it is remembered that this work is a carefully classified index of the whole of medical literature, not only books but pamphlets and original articles in the medical journals, and that on a reference to it any articles which have been written on medical subjects can be easily found, it will be seen that the cessation of this work would be an incalculable loss. Our correspondent's suggestion that £400 a year should be subscribed by this country is well worth the consideration of the profession and the medical societies, and as such we commend it to them."

"To all who have profited by the wealth of information in the Index Medicus, the news that its publisher, George S. Davis, has felt obliged to discontinue its publication will be read with regret. It seems that since 1885 the annual loss which has been borne by Mr. Davis has been from $500 to $1000, and that this year it bade fair to reach $2000, and no one can blame him for discontinuing his philanthropy. An endeavor was made by his friends to raise the number of subscriptions to a paying basis, but unfortunately was unsuccessful. The JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION states, however, that if its subscription list could be placed at twelve thousand that publication could and would carry on the Index with profit, and it is to be hoped this may be done. No one who has used the Index can fail to appreciate its value."—*The Atlantic Medical Weekly*, July 13, 1895.

Transmission of Tubercle Bacilli.—At the *Société de Biologie* of Paris at its session of June 29, 1895, MM. Bar and Rénon reported that they had undertaken the elucidation of the question of the direct transmissibility of the tubercle bacillus of Koch, through the placenta, to a human fetus. In five cases they took at the moment of birth of the child a certain quantity of blood from the umbilical vein, and injected it under the skin of abdomen of guinea pigs. In three cases the results were negative, and the guinea pigs did not become tuberculous, but in the remaining two, results were positive. In the first case the mother was in the third stage of phthisis, the sputa contained the bacilli. The placenta in this case presented no apparent lesions. The blood from the umbilical vein was injected into a guinea pig, which showed a chancrous ulcer at point of inoculation, and died two months later, with tuberculosis of the liver and spleen. The spleen pulp and the caseous masses of the chancrous ulcer contained the bacillus. The autopsy of the stillborn infant was practiced immediately after its expulsion; the result was

1 Bulletin Medical, No. 47, 1895.
2 Bulletin Medical, No. 47, 1895.

1 Le Province Medicale, No. 25, 1895.